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OCCUPATIONAL PSYCHOLOGY

OCTOBER, 1955 VOLUME 29 No. 4

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Occupational Psychology is published quarterly in January, April, July and October by the National Institute of Industrial Psychology. From January 1932 to December 1937 the Institute's Journal appeared monthly under the title *The Human Factor*; from January 1922 to December 1931 it appeared quarterly under the title *The Journal of the National Institute of Industrial Psychology*. The subscription rate for *Occupational Psychology* is 30/- a year. The National Institute of Industrial Psychology is a scientific association whose aim is the development of occupational psychology in Great Britain. Particulars of its work and information about membership can be obtained on application to the Secretary, NIIP, 14 Welbeck Street, London, W.1.

OCCUPATIONAL
PSYCHOLOGY

JANUARY 1955 VOLUME 29: NUMBER 1

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A Method of Interviewing used in Studies of Workers' Attitudes: I. Effectiveness of the Questions and of Interviewer Control

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In a recent paper (Marriott, 1953) a description was given of the methods used by this Group in an inquiry into satisfaction and discontent among production workers in industry. Over 1,600 workers were interviewed in two motor car factories (A and B) and in six rolling mills and three factories of a large engineering group (C) with a common management. It is not proposed to repeat here the details of the introductory and preparatory procedures used, but it should perhaps be stated that not only were the workers interviewed but their representatives, supervisors and managers were also seen individually and in groups, and every care was taken to ensure that all were informed and, as far as possible, approved of the investigation. In addition, frequent visits were made to the workshops so that the investigators became known to the workers and were themselves made familiar with factory conditions and operations. Various records were studied and analysed and, as stated in the previous paper, these exploratory and explanatory procedures were considered to be vital to the successful outcome of the investigation.

DESCRIPTION OF INTERVIEW METHODS

This paper is not concerned with the overall results of the inquiry but with an appraisal of the method used in interviewing the workers, the aim of which was to combine what seemed to be the best features of the 'free' and 'controlled' methods so that:

(i) the worker would have time and freedom to express his own viewpoint,

(ii) he would give his views about all the main factors in the working situation and not merely those which were uppermost in his thoughts at that particular time,
(iii) he could make an assessment of his own attitude and be given an opportunity to correct any bias he had shown,
(iv) the interviewer would not be at liberty to disturb the situation by spontaneous and possibly misleading or suggestive questions,
(v) the interview would not become merely a list of questions which by jumping from one topic to another would tend to produce perfunctory replies.

Previous experience had suggested that discussion with workers ranged round eight main topics: the operation or actual task performed, hours of work, shift system, amount of wages, payment system, management and their policies, supervision and workmates. It was felt that within this framework all the main worker problems could and would be raised. It was decided to treat each topic as a separate section of the interview and to link them with questions so worded as to switch the discussion smoothly from one to the other. The sequence was arranged so that the topics about which there was likely to be some diffidence came after the interview had warmed up and confidence had been gained.

(a) *The interview questions*

To illustrate the type of questions asked, the section relating to the 'operation' (or actual task performed) is reproduced below. Questions were of course different in detail in each section, but the same pattern was preserved:

(1) "You work in .... department, what is your actual job?"
(2) "How long have you been on this job?"
(3) "What do you like about ....?" (*Insert name of job given in reply Question 1*)
(4) "What do you dislike about ....?" (*repeat name of job*)
(S.A.1) "Which one of these statements would you say is the right one for you?" (*presented visually*)

I like my work very much
I like my work quite well
On the whole — I neither like nor dislike my work
I dislike my work to some extent
I dislike my work very much

(5) "Why do you say ...... (*repeat wording of item chosen in S.A.1*)?"

The questions which introduced each section (*e.g.*, 1 and 2) were essentially factual and the answers could be checked by observation or by reference to factory records. By placing them at the beginning of each section it was thought that the worker would be made more at ease as he discovered that they were within his ordinary range of experience and in no way constituted a test of intelligence or knowledge.

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The next questions (3 and 4) on likes and dislikes formed the pivot of the questionnaire and were open-ended and free from any suggestion. Interviewers were instructed to refrain from adding questions or probing further and to give the worker full freedom to talk for as long and as widely as he liked about the topic. Following these was a visual question (S.A.1) in which opportunity was given for self-assessment on five degrees of satisfaction with the topic under discussion. While placing a typewritten card in front of the worker, the interviewer said: " Which one of these statements would you say is the right one for you? " These self-assessments aimed at making the worker think again about his attitude to the topic as a whole rather than to some restricted aspect of it. He was then asked in the actual words he had used (question 5) why he had made that choice, for instance, " Why do you say you like your work quite well? " This gave him an opportunity to counter or explain any bias which had been shown without his being aware that this was the object of the question and he frequently developed his views still further.

In addition to the basic questions on likes and dislikes a varying number of closed and open-ended questions were asked where the conditions required it. At the end of the interview a final visual question was put to the worker regarding his general satisfaction with the whole working situation. As before, he was asked why he had chosen the particular item and in this way was given an opportunity to sum up his attitude. On the average the interview lasted from 45 to 60 minutes.

(b) *Sampling of workers*

The samples of workers were obtained by taking every *n*th individual from a nominal roll. Of necessity the samples varied in proportion and consisted of 340 men in factory A, 291 in factory B and 628 men and 407 women in factory C—a total of 1,666 workers. As records were required of each person's output, earnings and absence in the period prior to the interviews, those with less than one year's service were excluded in factories A and B and those with less than three months in factory C.

About ten per cent. of the original sample were not available for interview because of long-term absence, transfers to other departments, discharges or occasional errors in the nominal rolls, and a further one to two per cent. were unwilling to meet the interviewers. They were replaced by substituting the next name on the nominal roll. Only one worker declined to continue the interview after the investigator had explained the nature of the inquiry to him—and each one was given the opportunity to do so. This is some indication of the success of the introductory meetings and explanations in allaying possible suspicions and in promoting co-operation.

On the whole we feel satisfied that the workers interviewed were representative of the populations from which the samples were drawn.

(c) *Analysis of data*

The replies of the workers were recorded verbatim and the following is a brief outline of the way in which the interview records were analysed. As already stated, there were three types of questions:

(i) *Open-ended questions* (*on likes and dislikes*)

These questions yielded a wide range of replies of various lengths. Each response was divided into as many comments as could be reasonably distinguished—and on each topic a first classification was made into 'favourable', 'unfavourable' and 'neutral' comments. The favourable and unfavourable comments were then grouped under a number of subheads, of which the following for the 'operation' are examples:

FAVOURABLE	UNFAVOURABLE
Interest	Monotony
Familiarity	Unfamiliarity
Simplicity	Complexity
Physical comfort	Physical discomfort
Social aspects	Social aspects
Financial aspects	Financial aspects
Preferences	Preferences

An example from an interview record may help to illustrate this method of analysis. To the questions on likes and dislikes for the operation one worker replied:

(Question 3) "The job is tidy, clean and light. It requires a certain amount of skill in setting up the job and getting a proper finish."

(Question 4) "The monotony of the job—we are doing the same thing day after day."

This was analysed as three favourable comments on physical comfort, one favourable comment on interest and one unfavourable comment on monotony. Within each of the subheads shown above, the various aspects such as 'cleanliness' and 'lighting' in this example were kept separate.

In order to ensure consistency in analysis, a detailed code was prepared for each topic showing under each subhead the separate aspects which had been distinguished and the range of comments which were considered to be variations of each aspect. It is not claimed that subjective judgments in the process of classification were completely eliminated, but any slight inconsistencies did not affect the broad comparisons made later in this paper.

In order to compare the replies to these questions, the proportion of favourable comments (PFC) was used as a measure. This is the ratio of favourable to total comments after excluding the very small number (fewer than one per cent.) of neutral or ambiguous answers. As the same questions were asked of each worker, this provides an approximate indication of the level of satisfaction. The assumption of equal weighting for each comment is an oversimplification but this is offset to some extent when, as in this inquiry, group attitudes are under consideration and the importance of the topics or aspects of them varies from worker to worker and from favourable to unfavourable comments.

(ii) *Self-assessments*

Comparisons between groups of workers have also been made according to their distributions on the five point scale used in the self-ratings. It was also found useful to calculate for each topic a group ' index of satisfaction ' (based on a suggestion by Lazarsfeld and Robinson (Lundberg, 1942)) by allotting to the assessments the following values: 'very satisfied' +2, 'satisfied' +1, 'neither satisfied nor dissatisfied' 0, 'dissatisfied' −1, and 'very dissatisfied' −2 and dividing the total by the number of workers. This index has been used to indicate broad similarities or differences between groups and has a qualitative rather than a quantitative value.

(iii) *Closed-end questions*

In the analysis of questions which required a 'Yes' or 'No' answer or a choice between alternatives, such as " Do you feel secure or insecure in your job here? " " Why? " workers were first grouped under the broad headings of 'secure', 'insecure' and 'doubtful' and the proportions making each response were compared for different groups of workers. Any reasons given for the attitude were entered under each of these headings.

THE EFFECTIVENESS OF THE QUESTIONS

Great care was taken to put the workers at their ease and, in the course of the introductory questions, to direct their attention to each topic in a natural way. It is a reasonable assumption that they responded freely to questions on likes and dislikes of a situation with which they were so familiar, the more so when they were given a second opportunity after each self-assessment to enlarge on or modify the comments previously made. As this inquiry was concerned mainly with the attitudes of large groups, individual differences in fluency were not important and the responses of the groups usually covered all aspects of the working situation.

In judging the effectiveness of the questions it is necessary to consider (a) two problems concerned with the self-rating scales — the number and order of presentation of the items and (b) the relationship between the self-assessments and the likes and dislikes expressed.

(a) *The self-assessments*

As noted earlier, two of the steps in the scales were concerned with great and moderate satisfaction, two with their opposites, and at the mid-point of the scale was a statement to cover neutral or ambivalent attitudes.

(i) *Number of items in the scale*

Previous experience had suggested that on such topics and with inexperienced subjects a five point scale was within the discriminative ability of the average worker and was likely to yield reliable results. Guilford (1936) quoted from early experimental work which showed that five or seven steps are likely to be most profitable. In a recent experiment, Bendig (1953) has suggested that " the reliability of group or individual self-ratings is little affected by variations in the number of scale categories within the limits of from 3 to 9, but both individual and group reliability begins to decline when 11 categories are used". In the present inquiry all the interviewers were agreed that the scale categories did not call for finer judgments than the worker was capable of making.

(ii) *The order of presentation of the items*

In the factories A and B and C^x* the five choices were presented in descending order from high to low satisfaction. To test for possible space error in this arrangement an experiment was carried out in factory C^y in which 141 men and 407 women were interviewed and two series (X and Y) of visual cards were used. The choices in series X were presented in the normal 'descending' sequence on the alternate topics of operation, payment system and general attitude, while those on the intermediate topics of wages and firm were presented in 'ascending' order from low to high satisfaction. In series Y the sequence was the reverse of series X for all topics except general attitude in which, as a control, the normal descending sequence was retained. Throughout the investigation each worker was allotted a reference number and in this experiment those with even numbers were presented with series X and those with odd numbers with series Y cards. By this means the samples were split into similar halves. Table 1 shows the distributions of the self-ratings obtained from the normal and reversed order of presentation.

TABLE 1: THE DISTRIBUTION OF WORKERS (IN FACTORY C^y) ACCORDING TO DESCENDING (D) AND ASCENDING (A) PRESENTATION OF SELF-ASSESSMENT SCALES

	OPERATION				WAGES				PAYMENT SYSTEM				FIRM				TOTAL		GENERAL SATISFACTION			
	MEN		WOMEN		MEN		WOMEN		MEN		WOMEN		MEN		WOMEN				MEN		WOMEN	
Series ..	X	Y	X	Y	Y	X	Y	X	X	Y	X	Y	Y	X	Y	X			X	Y	X	Y
Order ..	D	A	D	A	D	A	D	A	D	A	D	A	D	A	D	A	D	A	D	D	D	D
VS† ..	26	23	92	91	9	8	52	45	10	8	39	29	32	22	89	82	349	308	23	17	68	62
S ..	30	27	82	79	14	18	62	73	31	29	91	95	32	40	100	103	442	464	39	44	117	120
N ..	9	16	22	24	39	39	75	69	22	27	61	69	5	7	15	17	248	268	3	7	15	19
D ..	5	5	7	10	7	3	11	13	7	4	10	10	2	1	0	1	49	47	5	3	3	2
VD ..	0	0	0	0	2	2	4	3	0	3	2	1	0	0	0	0	8	9	0	0	0	1
Total ..	70	71	203	204	71	70	204	203	70	71	203	204	71	70	204	203	1096	1096	70	71	203	204

†VS, very satisfied; S, satisfied; N, neither S nor D; D, dissatisfied; VD, very dissatisfied.

The distributions were very similar whether the choices were presented in ascending or descending order and there were no significant differences. Tested by the chi-square method 'P' values were in no case less than .20 and ranged up to .80. There was however a slight but consistent tendency for fewer 'very satisfied' assessments to be made when the scale was presented in ascending order and there were correspondingly more 'satisfied' and 'neutral' assessments, the number of dissatisfied workers being unaffected. This tendency was not serious and did not affect the interpretation of the results.

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*In the last three departments (C^y) of factory C, self-assessments were used for only four topics (see Table 1) as the investigation was concerned mainly with the payment systems and more time was spent on that topic. Hence the division of factory C into C^x and C^y for present purposes.

On the whole, the experiment demonstrated that the workers were taking into account all the steps in the scales before selecting those which corresponded to their attitudes.

(b) *Relationship between likes and dislikes and self-assessments*

It seems reasonable to assume that a worker, within the limits of his analytical ability and his working experience, would be likely to assess his attitude more accurately after discussing what might be termed the manifest causes of it than if he had been asked first to rate himself on the five-point scale. The following tables show the amount and consistency of agreement between the two measures—likes and dislikes and self-assessments.

The first test of relationship was made by comparing the proportion of favourable comments (PFC) with the self-assessments (SA) and it was found that for each topic the PFC was highest for SA(i) (very satisfied) and lowest for SA(v) (very dissatisfied), with a consistent descending order for the intermediate assessments. In Table 2 examples are shown for the first topic discussed (the operation) and for all the topics combined.

TABLE 2: PROPORTION OF FAVOURABLE COMMENTS FOR EACH SELF-ASSESSMENT

FACTORY		SELF-ASSESSMENTS					
		VS	S	N	D	VD	ALL WORKERS
Operation	A	.82	.65	.44	.43	.14	.63
,,	B	.79	.69	.36	.21	.19	.59
,,	C*x*	.80	.71	.43	.27	.08	.65
,,	C*y*	.87	.76	.46	.35	—	.76
All topics	A	.87	.72	.43	.26	.11	.72
,,	B	.91	.75	.40	.21	.10	.65
,,	C*x*	.87	.75	.37	.21	.09	.72
,,	C*y*	.91	.82	.44	.26	.14	.77

These show very consistent trends as do the results for the other topics. A further test was made by studying the relationship between the two measures for individual workers. The PFC's were divided into five equal categories which were arranged according to the corresponding SA's, as shown in the following example (Table 3):

TABLE 3: DISTRIBUTION OF WORKERS ACCORDING TO SA AND PFC (FACTORY A — OPERATION)

PFC	SELF-ASSESSMENT					
	VS	S	N	D	VD	TOTAL
1.00 — .81	(51)	38	1	0	0	90
.80 — .61	35	(47)	13	2	1	98
.60 — .40	8	44	(26)	7	0	85
.39 — .20	1	13	21	(10)	0	45
.19 — .00	0	2	10	8	(2)	22
All categories ...	95	144	71	27	3	340

With a perfect correlation all the workers would, of course, have been found in the bracketed cells, but the imperfections of the interview instrument as well as the arbitrary division of the PFC into five categories have reduced this considerably. It is, however, interesting to note that in this example the degree of complete coincidence was 40% and partial coincidence was 89% (*i.e.*, including those who fell within the bracketed and immediately adjacent vertical cells). The rank correlation (Tau: Kendall, 1948) in this case is 0.55.

The relationship was tested still further by summating the comments on all topics and calculating the PFC in the whole interview for each worker. Similarly the SAs were totalled and an average self-assessment index was calculated for each worker. The product moment correlation between these measures for each interviewer is given in Table 4.

TABLE 4: PRODUCT MOMENT CORRELATIONS (FOR EACH INTERVIEWER) BETWEEN PFC'S AND SA'S

FACTORY	INTERVIEWERS						
	S	K	B	P	C	D	W
A	.67						
B		.75					
C*x*			.72	.75	.70	.71	.66
C*y*			.70	.77	.64	.62	.59

All these correlations are significant at the .01 level. Thus they show a very consistent and significant tendency both as regards the separate factories and the individual interviewers.

These comparisons between the comments made by the workers and their subsequent self-assessments on the five-point scale show broad agreement between the two measures and indicate that, in general, the workers were able to assess their attitudes to the topics with reasonable consistency. The correlations also show that any personal bias the interviewers may have had did not affect the relations between the two measures. (The general question of interviewer differences is dealt with in the next section.)

THE INFLUENCE OF THE INVESTIGATORS

(a) *Comparison of interviewer results*

Among those concerned with the assessment of attitudes by verbal methods, one of the main problems is the influence of the person conducting the investigation. The effect of the interviewer appears likely to increase in proportion to the amount of freedom allowed to him. Interviewer differences are difficult to measure and if only one interviewer is involved, as often happens, his effect on the responses of the worker cannot be judged. In these investigations where, after the early stages, two or more interviewers dealt with similar interview sub-samples and where precautions were taken to ensure question control and adequate preparation it is possible to compare the results obtained by the different interviewers.

During the course of these investigations seven interviewers were engaged on the work but for only five of them is it possible to make useful comparisons.* None of them took part in the whole investigation but at no time were there less than two simultaneously engaged and for much of the time there were three or four. As stated earlier, the factory samples were obtained by selecting every *n*th worker from the nominal roll and these were divided amongst the interviewers by taking alternate workers or, according to the number of interviewers, every third or fourth. As a result, the interviewer sub-samples were on the whole similar as regards age, length of service, type of work, workshop environment and supervision.

All five interviewers were males in the 25-35 age group and had previous training in psychology or interviewing or both. While none had experienced quite this kind of interviewing, it was not deemed necessary to provide any elaborate training. A discussion of principles and procedure was followed by a demonstration of interviewing of non-sample workers by the team leader. After this, in his presence, several trial interviews were conducted by the interviewer and any weaknesses in manner or method were pointed out and discussed. This was followed by interviewers sitting in with each other for a few trial interviews. After some experience of interviewing alone the instructor again sat in on further non-sample interviews and results were discussed. To supplement this practical experience, available literature on interviewing was read and the interviewers constantly compared notes and profited by mutual criticism.

Thus, all reasonable precautions were taken to control the interview situation. In these circumstances comparison of the results obtained by the interviewers should reveal any gross effects of their influence.

(i) *Self-assessments*

Table 5 shows the interviewers arranged in pairs with comparable samples and indicates the numbers of workers and topics involved in each comparison of the self-assessments. (As an illustration of the effects of interviewers dealing with dissimilar samples, factory A has been included at the foot of the table.)

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*In factory A, the two interviewers, who took no further part in the interviewing, began on different dates and dealt with workers from different departments.

TABLE 5: INTERVIEWER COMPARISONS BY SA'S

FACTORY	INTERVIEWERS (NO. OF WORKERS)		NO. OF TOPICS	
			SA'S	DIFFERENCE SIGNIFICANT
B and Cx ...	B(194)	P(194)	9	1
Cx	P(135)	C(136)	9	1
	P(71)	D(71)	9	
	P(48)	W(48)	9	
	C(71)	D(71)	9	3
...	C(48)	W(48)	9	
	D(48)	W(48)	9	
Cy	C(170)	D(166)	5	
	C(170)	W(167)	5	
	D(166)	W(167)	5	
A	S(194)	K(129)	9	3

Thus for each pairing of interviewers, nine comparisons of the self-assessments were made in factories A, B and C^x and five in factory C^y. Chi square tests were used and differences were considered significant at the 5 per cent. level.

Table 5 shows that in only five of the 78 tests in which interviewers had comparable samples were the differences significant and three of these were between interviewers C and D. The latter result is similar to that in factory A where the sub-samples were quite different. An explanation was sought on similar lines and it was discovered that, in one of the three mills concerned, there was a division of work which was not apparent before sampling. The rolling machines were located in the workshop according to size, in two sections known as the 'Big side' and 'Little side'. Of all the mills in factory C^x this was the least modern and there was much manhandling of material especially on the 'Big side' where it was more unwieldy and had to be handled more frequently. Some time was lost waiting for 'bogeys' on which the material was moved, and no payment allowance was made for this . In a later interview the mill manager stated that the system of payment was "inequitable" and that it was corrected after the workers had been interviewed. Examination of the interviewer sub-samples in this mill showed that they were similar for all variables except this one concerned with type of work:

Interviewer	*No. of workers interviewed*		
	'Big side'	*'Little side'*	*Total*
P	5	3	8
C	7	2	9
D	3	11	14

The 'Little side' was found to be significantly more satisfied than the side' with the operation, wages and payment system and it was on the first and last of these that interviewers C and D had different results as well as on the hours of work, which were identical; this may be a reflection of operational fatigue. The differences between interviewers P and D showed a similar tendency though these were not significant.

Thus in this case as well as in factory A it seems that the differences can be ascribed to sample rather than interviewer differences. It was the first mill or factory in which more than two interviewers were engaged and in subsequent mills such differences in the sub-samples were avoided by stratifying for type of work and payment systems both of which were diversified in these mills.

(ii) *Open-end questions*

Table 6 shows the proportion of favourable comments (PFC) and number of comments per interview obtained by each interviewer (paired horizontally). The differences are significant only where indicated:

TABLE 6: INTERVIEWER COMPARISONS BY (i) PFC AND (ii) MEAN NUMBER OF COMMENTS (LIKES PLUS DISLIKES) PER INTERVIEW

FACTORY	INTERVIEWER (NO. OF WORKERS)	PFC *i*	NO. OF C'MENTS *ii*	INTERVIEWER (NO. OF WORKERS)	PFC *i*	NO. OF C'MENTS *ii*	DIFFERENCE SIGNIFICANT(S) *i*	*ii*
B and C*x*	B(194)	.68	22.50	P(194)	.66	23.26	—	—
C*x* ...	P(135)	.73	25.06	C(136)	.70	20.60	—	S^1
	P(71)	.71	25.22	D(71)	.73	24.54	—	—
	P(48)	.73	23.75	W(48)	.73	23.06	—	—
	C(71)	.68	20.43	D(71)	.73	24.54	—	S^2
	C(48)	.70	20.38	W(48)	.73	23.06	—	S^3
	D(48)	.73	23.46	W(48)	.73	23.06	—	—
C*y*	C(170)	.76	10.90	D(166)	.76	10.37	—	—
	C(170)	.76	10.90	W(167)	.77	11.40	—	—
	D(166)	.76	10.37	W(167)	.77	11.40	—	S^4
A ...	S(194)	.77	26.40	K(129)	.66	25.78	S^5	—

In S^1, difference between means (4.46) nearly 8 times S.E. of difference (.59)
In S^2, (4.11) 5 (.86)
In S^3, (2.48) 3 (.86)
In S^4, (1.03) approx $2\frac{1}{2}$ (.40)
In S^5, (.115) over 4 (.027)

The table shows that the proportion of favourable comments was closely similar for paired interviewers except in factory A where the interviewer samples were not comparable. The PFC is remarkably consistent throughout and demonstrates that on the average in these factories between two-thirds and three-quarters of the comments made were favourable.

There were, however, some differences between interviewers in the amount of information obtained. This is seen in the columns giving the mean number of comments per interview. Thus, of the eleven possible comparisons four showed significant differences and in three of these interviewer C elicited fewer comments than his colleagues. This particular difference did not persist however into the later part of the investigation in the C^y factories, though it appeared between the other two interviewers. (This last instance is on the borderline of significance.) It is possible that experience, discussion and mutual criticism reduced the gap between the interviewers. In other words, the recognition of this difference coupled with a knowledge of past results may have had an influence. This last argument assumes that the interviewer was able to exercise some control, for example, over the degree of rapport established or, more likely, over the written record of the interview.

It is interesting to note that these differences had no effect on the proportion of favourable comments (Table 6) and, therefore, on the general balance of favourable and unfavourable attitudes. More detailed comparisons can be made by considering the nature of the comments elicited by the interviewers on each topic. For instance, the replies to the questions on the payment system were tabulated under such subheads as, incentive; relations with others; rates, standards and earnings; performance or organisation of job; structure; preferences (for own or other systems); general comments. Perusal of the analysis sheets shows that the interviewers were broadly agreed on the numbers of workers expressing likes and dislikes under the various subheads; inevitably there were a few differences, but these rarely affected the interpretation of the results.

It can be said with some certainty that the differences exhibited by the interviewers using this method were not of sufficient magnitude to bias the results or their interpretation. Though the comments differed at times in quantity and slightly in nature within a given topic, the balance of favourable and unfavourable replies was remarkably consistent.

(b) *The effects of the presence of the investigators and the influence of the workers on each other*

To test adequately whether the presence of the investigators or the conscious or unconscious influencing of the workers by each other had any effects on the results as the work proceeded would require a more elaborate experimental design than was possible in this inquiry. In the introductory meetings and in the interviews, each worker, representative and supervisor was asked to encourage others to attend for interviewing and, at the same time, the importance of not divulging the questions to them was explained and emphasised. Many general inquiries were made as to the effectiveness of this appeal and those who came to the interviews were often asked about it. From these sources there was much evidence of co-operation and none of collusion—rather the reverse.

Unfortunately the need for testing this was not sufficiently foreseen and arrangements were not made to match samples of workers at the beginning and end of the interviewing periods. Consequently such factors as the operation, shifts, earnings, and supervision were not always sufficie controlled for these purposes. However, any differences that may have been due to this have been minimised by analysing the data by departments; these were physically and functionally separate so that even if the workers were aware that the inquiry was proceeding, they were little, if at all, affected by it until it reached their own departments. The interview records were arranged in date order and grouped in four successive periods with equal numbers of workers in each; thus the first period contains the first quarter and the fourth period the last quarter of workers interviewed. The time of the departmental interviewing programmes ranged from several days to about four weeks. In Table 7 the results have been combined into factory units and are shown in terms of indices of satisfaction on each topic for each period.

TABLE 7: INDICES OF SATISFACTION FOR INTERVIEWED WORKERS GROUPED IN FOUR SUCCESSIVE PERIODS

FACTORY	NO OF WORKERS	PERIOD	TOPICS								
			OPERATION	SHIFTS	HOURS	WAGES	PAYMENT SYSTEM	FIRM	FOREMEN	WORKMATES	GENERAL ATTITUDE
A ..	85	1	.79	.87	1.45	1.34	.62	1.52	1.67	1.51	1.61
	85	2	.95	1.01	1.52	1.38	.69	1.46	1.59	1.58	1.67
	85	3	.87	1.09	1.38	1.31	.42	1.36	1.46	1.54	1.65
	85	4	.94	.96	1.41	1.32	.71	1.31	1.41	1.54	1.61
	340	Total	.89	0.97	1.44	1.34	.60	1.41	1.54	1.55	1.64
B ..	73	1	.79	—	.15	1.01	.85	1.14	1.40	1.51	1.33
	72	2	.58	—	.15	1.03	.79	.81	1.35	1.25	1.26
	73	3	.85	—	.04	1.06	1.16	1.15	1.41	1.40	1.29
	73	4	.71	—	.03	.21	1.08	1.01	1.40	1.42	1.42
	291	Total	.74	—	.07	1.07	.97	1.03	1.39	1.39	1.33
Cx ..	122	1	.93	.91	1.18	.49	.59	1.20	1.40	1.43	1.23
	122	2	1.16	1.02	1.21	.56	.55	1.43	1.40	1.37	1.23
	121	3	.91	1.07	1.23	.57	.55	1.32	1.36	1.27	1.14
	122	4	.79	.64	1.16	.52	.45	1.26	1.26	1.39	1.12
	487	Total	.95	.91	1.21	.54	.54	1.31	1.35	1.37	1.18
Cy ..	137	1	1.26	—	—	.68	.76	1.37	—	—	1.26
	137	2	1.11	—	—	.59	.63	1.25	—	—	1.18
	137	3	1.17	—	—	.65	.67	1.31	—	—	1.15
	137	4	1.25	—	—	.55	.69	1.35	—	—	1.14
	548	Total	1.19	—	—	.62	.69	1.32	—	—	1.18

Though there are differences in the level of satisfaction between the periods, there is no discernible pattern in the increases or decreases which would indicate effects due to the presence of the investigators or to the influence of workmates or others. On the whole, and considering these groups were not matched, there is a considerable degree of consistency in the four periods. Using chi square in comparing the first with each of the other periods, there were only 3 out of 95 tests which showed significant differences at the .05 level; they were:

Factory B : Hours —1st and 4th periods
,, B : Workmates —1st and 2nd ,,
,, C^x : Firm —1st and 2nd ,,

In the first and third of these, satisfaction increased and in the second it decreased. It is possible that comparison between periods 2, 3 and 4 would also reveal some differences, but as already stated there is no indication of a regular trend in either direction.

It is, of course, likely that these factory figures are masking differences within the departments. As departmental samples of workers were small, they were arranged in two equal groups by date order. Of the 231 possible comparisons on the interview topics only 22 were significantly different and, in 12 cases, the workers in the second half were more satisfied and, in 10, they were less satisfied than those in the first half. A careful examination of these differences shows that they were due mainly to dissimilarities in conditions of work. Examination of the comments made in answering questions on likes and dislikes also shows no evidence of differences which would affect group attitudes at the end as compared with the beginning of the interviewing periods.

Thus the available evidence from these data suggests that the investigators in their interviewing and in their continued presence in the factory had no serious effect on the results obtained. Also, there is no indication that the workers or their representatives had any deliberate or unconscious influence on each other.

This paper will be concluded in the next issue of this Journal by a discussion of the validity of the interview method and a general appraisal of the results.

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Comparison of Paced and Unpaced Performance at a Packing Task

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SUMMARY

In paced work (*e.g.*, conveyor belt systems) activity tends to consist of a period of action followed by a period of waiting and so on. When items requiring action arrive at equal time intervals, the duration of the waiting period will depend on the duration of the previous action period. If an action period takes longer than the interval between items, then there is the possibility that an item will either wait for the operator, or it will go by 'unprocessed'. These considerations led to the hypothesis that the output of industrial conveyor belt systems would be less than that expected from knowledge of the mean processing time.

An experiment to test this using five experienced operatives as subjects was carried out on the shop floor of a factory. Three specific questions were examined: (a) What is the nature of the distribution of cycle time when the work is unpaced? (b) If the same work is then paced at a rate based on the unpaced mean, is output adversely affected (1) when operatives carry out a complete cycle, (2) when the cycle is divided between two operatives? In the first of these two last conditions, a machine delivers work to an operative at equal time intervals. In the second, a machine delivers work at equal time intervals to one operative, who carries out part of the cycle, and then passes the work on to another operative who completes the cycle.

The mean cycle time in the unpaced condition showed no significant change in length throughout the day, but the coefficient of variation (used as a measure of scatter) increased consistently and without exception, the change from the beginning to the end of the day being from 0.19 – 0.22 which is statistically significant at better than the 0.001 level. Under the paced condition with one operative completing the cycle, output was as good as in the unpaced condition. When the work was divided between the two operatives output fell by about 12%.

The partial failure of the original hypothesis is considered to depend, to some extent, on certain unavoidable differences in the working conditions introduced when changing from unpaced to paced, but largely on the relationship between the cycle time distribution and the linear speed of the conveyor belt. This point is discussed in detail and it is argued that: (a) when the mean cycle time and the distribution are constant, increasing

the belt speed (not the rate of work) will decrease output; and (b) when the mean cycle time and the belt speed are constant, an increase in the size of the coefficient of variation will decrease output. This latter point is obviously relevant to the training of operatives and to the noted increase in the scatter of cycle times during the working day.

A method is mentioned whereby, under any condition, output would be dependent only on operatives' mean cycle time.

INTRODUCTION

It is well known that the time required by a person to repeat even a very simple operation is not normally distributed, but is skewed towards the long end. Very little study of the true nature of this distribution has been made with reference to industrial operations, in spite of the fact that in planning industrial processes it is often important to know the average value of operation times, and the reliability of that value. Quite often in practice the distribution is treated as if it were normal, the average being accepted as the most representative value.

The writer (Conrad, 1954) has recently drawn attention to the relevance of the distribution of operative cycle time, as distinct from the average time, to the organisation in industry of machine-paced work. It was pointed out that, for example, in the case when a machine periodically delivers work for processing to an operative, the period being equal to the mean operative time, the operative will tend to be unable to deal with every item of work delivered. If the operative completes a cycle faster than the machine period, then he must wait idly for the next item; but if he takes longer than the machine period, then either the machine must stop or the item is liable to go by unprocessed. This is a simplification of the practical situation where frequently many processes are linked in such a way that weakness at one point in the system will affect the remainder.

In designing flow production systems the desired rate of flow has to be determined, and there is perhaps a risk that the maximum possible rate would be slower than the operative average rate. It would also follow that the output would be greater if the work became unpaced—*i.e.*, the operative was not required to keep up with a predetermined rate—and the operative never had to wait for material.

In the experiment to be described it was possible to consider two cases both of which are common in light industry. The first is when a machine periodically delivers work to an operative who carries out a complete process. The second is when the process is subdivided and the work is passed from machine to operative and then on to at least one other operative Ideally, the output in the two cases would be the same if the processing periods of the two operatives were always concurrent.

Three specific main questions were posed: (1) What is the nature of distribution of operative cycle time when the work is unpaced? (2) Is output affected when the paced rate is set at the mean value for the unpaced work? (3) Is output affected if the paced work at the given rate is now divided between two operatives?

The experiment was carried out in a department of a medium-sized factory. The relevant operation consisted of packing six jars into a carton in a particular way for display purposes. To prevent breakages in transport, the carton was designed so that the jars fitted very tightly, and thus some care was required in packing. Normally the operation was divided into three components and was duplicated so that packing was carried out by a team of six.

PART I: UNPACED WORK

Procedure

For a number of reasons it was decided not to attempt to change the three operations individually from paced to unpaced. Primarily because (a) major engineering work would have been involved; (b) the line handled a large proportion of the company's output of this particular product and it could not therefore be involved in its entirety too deeply in an experiment; and (c) it was obvious to the investigator that the existing paced rate resulted in considerable underloading of six operatives.

On the other hand, the company were generously prepared to set up and maintain suitable alternative plant which was not in general use. The main item of this was a semi-automatic labelling machine. This machine required hand feeding of jars one at a time and hand removal of labelled jars. But so long as the machinist depressed a foot pedal, labelling automatically continued at a fixed time interval.

There was thus available a controlled flow of jars which could be turned on or off by the labeller and it was arranged to feed this flow to a single operative who would pack complete cartons, whilst working at a table immediately beside the labeller. The roles of these two operatives were clearly defined and explained. The packer—who was the experimental subject—was to pack jars at her own pace without regard to the labeller, drawing jars from a standing queue on the table at which she worked. The labeller was instructed to deliver jars on to this table at such rates that there were never less than 12 jars on the table and never more than about 36. This was well within the capacity both of the labelling operative and machine. Ancillary labour was provided to ensure the supply of jars to the labeller, the removal of packed cartons, and the supply of empty cartons to the packer.

This equipment was placed in the packing room only a few yards from the main packing line. Although in this condition, the experimental subjects packed singly instead of in a group of six, they remained in fact a unit [illegible] closely linked group of operatives supplying and carrying away material. In no sense could they be considered to be socially isolated, except that they were of course the experimental subjects. At least three other operatives were within touching—and talking—distance.

Five operatives acted one at a time as subjects. They were selected by the forewoman and all volunteered when approached in this way. The grounds for selection were that they were all thoroughly experienced packers, and all had at some time worked on a line on which jars had been labelled by the semi-automatic machine. They were considered as a group to represent below average, average and above average skill. The youngest was aged 18 years and the eldest was over 50. During the course of the experiment none of the five were absent from work when scheduled on the programme to serve as a subject. At the beginning of the investigation a copy of this programme showing day to day subject requirements and experimental conditions was given to each girl.

Each of the five subjects worked for one day in each of two successive weeks. No one worked on the same day of the week on the two occasions. The normal working day was divided naturally by authorised breaks and certain other duties into four spells of approximately 1¾ hours each, beginning at 8 a.m., 10.15 a.m., 2 p.m. and 4.15 p.m., and the object of the first part of the experiment was to determine operatives' unpaced packing time (6 jars).

Two measures of performance were obtained:

(1) About the middle of each of the four daily spells, the duration of each of not less than 200 consecutive work cycles was recorded on paper moving at 1 cm/4 sec. The beginning and end of the cycles were signalled by an observer pressing and releasing a light manual bell-push type of switch. The cycle was defined as beginning when the operative reached for an empty carton, and as ending when she pushed the full carton along the packing table. This record when analysed provided the mean cycle time and its distribution of each operative on two days divided into four parts of the working day. Thus eight distributions were available for each operative based on 1,600-2,000 observations.

(2) At the beginning and end of each spell the operative in charge of the labelling machine recorded the time on the packing room clock, and the number showing on an automatic counter which was never zeroed, and which indicated the numbers of jars labelled. (The assumption that almost all of the jars labelled were then packed was quite safe.) She also recorded the durations of any interruptions of normal working, such as mechanical breakdown or failure in the supply of jars which occasionally happened.

Results

Table 1 shows the recorded mean packing time and the coefficient of variation of the mean of each operative during the four daily spells. Gr ing the four spells together, these values are also shown as a packing rate in terms of jars packed per minute. Every operative worked faster during her second day, but the accompanying increase in the coefficient of variation is clearly insignificant. These two facts taken together suggest a tightening up of a method of work rather than a radical change. Some examples of typical distributions of cycle time are shown in Fig. 1.

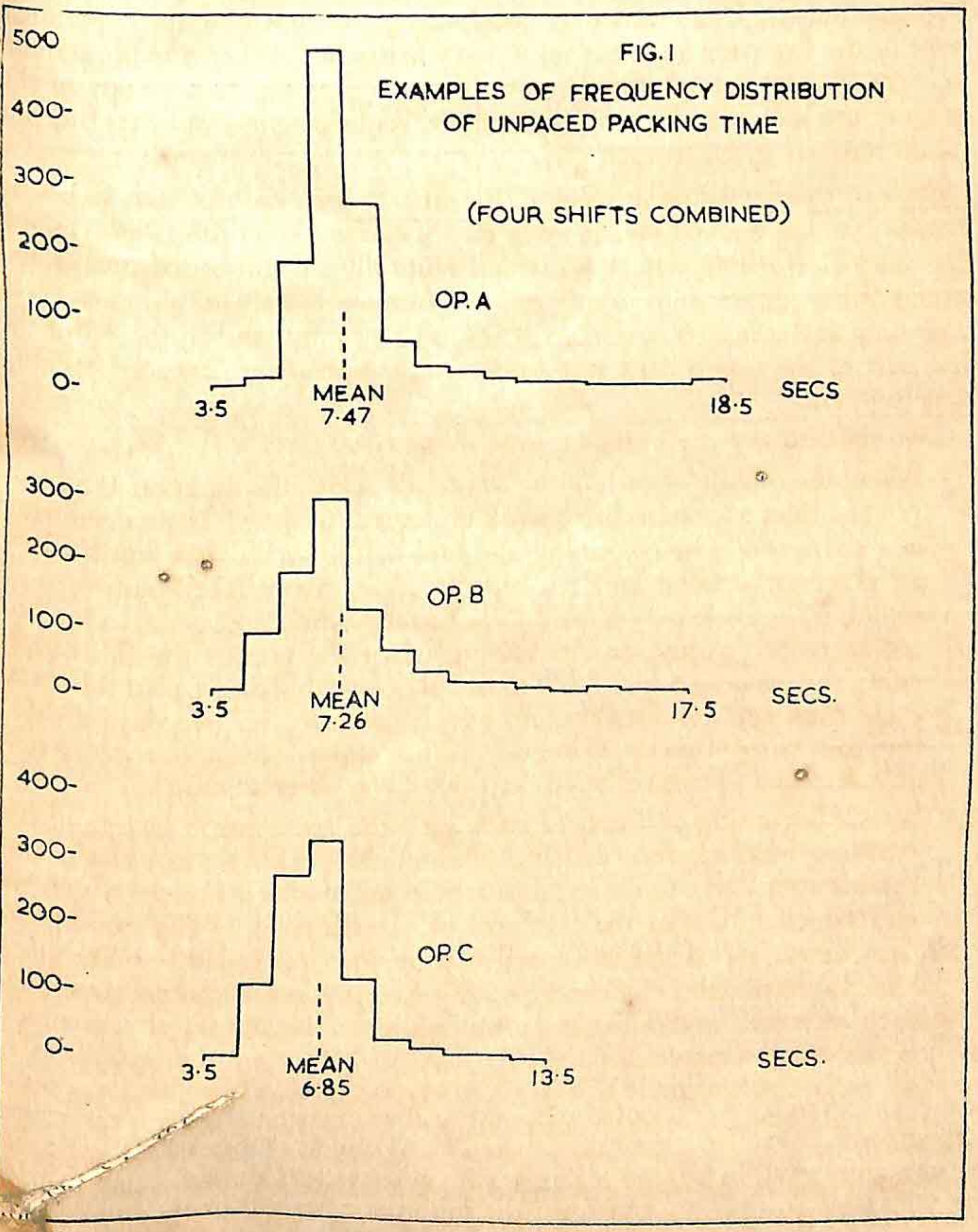

FIG. I

EXAMPLES OF FREQUENCY DISTRIBUTION OF UNPACED PACKING TIME

(FOUR SHIFTS COMBINED)

TABLE I: PART I — MEAN PACKING TIME, IN SECONDS
(coefficient of variation in brackets)

WEEK 1

SPELL OPERATIVE	1	2	3	4	MEAN NUMBER OF JARS A MINUTE
A	7.95 (.138)	8.67 (.186)	8.59 (.164)	8.19 (.173)	43.4
B	7.36 (.250)	7.58 (.208)	7.65 (.211)	8.03 (.273)	47.1
C	7.52 (.227)	7.12 (.198)	6.78 (.218)	6.90 (.246)	51.0
D	8.76 (.166)	8.43 (.202)	8.43 (.194)	8.56 (.226)	42.2
E	8.92 (.141)	9.22 (.184)	9.03 (.172)	9.18 (.181)	39.7
Mean ...	8.10 (.184)	8.20 (.195)	8.10 (.192)	8.17 (.220)	

WEEK 2

SPELL OPERATIVE	1	2	3	4	MEAN NUMBER OF JARS A MINUTE
A	7.39 (.177)	7.57 (.183)	7.55 (.200)	7.34 (.207)	48.2
B	7.20 (.247)	7.24 (.241)	7.22 (.236)	7.29 (.315)	49.6
C	6.85 (.211)	6.92 (.212)	6.78 (.212)	6.84 (.222)	53.3
D	7.43 (.146)	7.33 (.182)	7.47 (.207)	8.26 (.184)	47.3
E	7.71 (.156)	7.76 (.164)	7.95 (.147)	7.59 (.201)	46.4
Mean ...	7.32 (.187)	7.36 (.196)	7.39 (.200)	7.46 (.226)	

The pattern of performance change *during* any one day is quite different. Considering the two days together and comparing the first and last spells, the packing time shows no significant change. Out of the 10 cases, 5 show faster times and 5 are slower. But the coefficient of variation shows an increase in each of the 10 cases. The difference is statistically significant at better than the 0.001 level. Fig. 2 gives the trends during the four spells.

Although the investigators were present in the packing room most of the time, specific observation of the operatives occurred only during the times when recordings of packing times were being made. Operatives were relatively unobserved in a sense for approximately two-thirds of the total working time. By using the data from the automatic counter it was possibl compare the working rates during the periods of observation and during the remaining periods.

Four values are given in Table 2. The first is the net output. This is based on the recorded mean packing times. It is the output rate during the times that operatives were actually packing. The second value is the gross output rate during the same period. The difference reflects cessations of packing during the observation period for various reasons, most of them connected with the running of the labelling machine. The third value is the gross output rate during the periods when recordings were not being made. The indication is that what differences there are are small. The fourth value in Table 2 is simply the automatic counter reading divided by the number of minutes of work during the entire day.

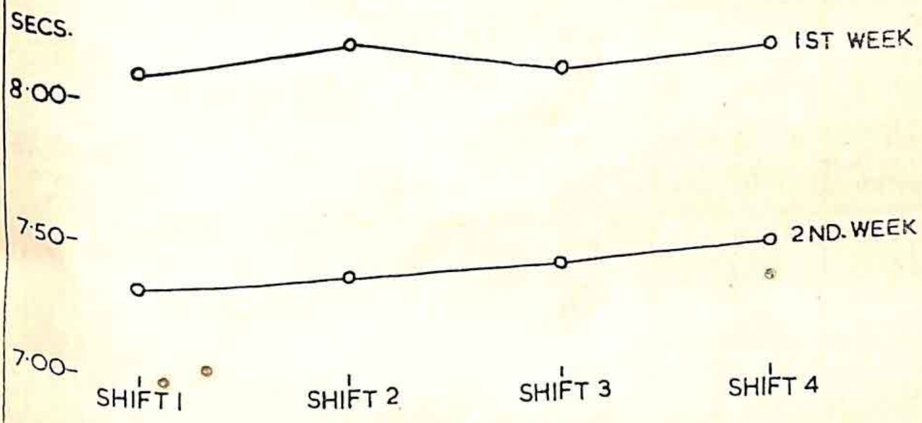

FIG. 2a. MEAN PACKING TIME.

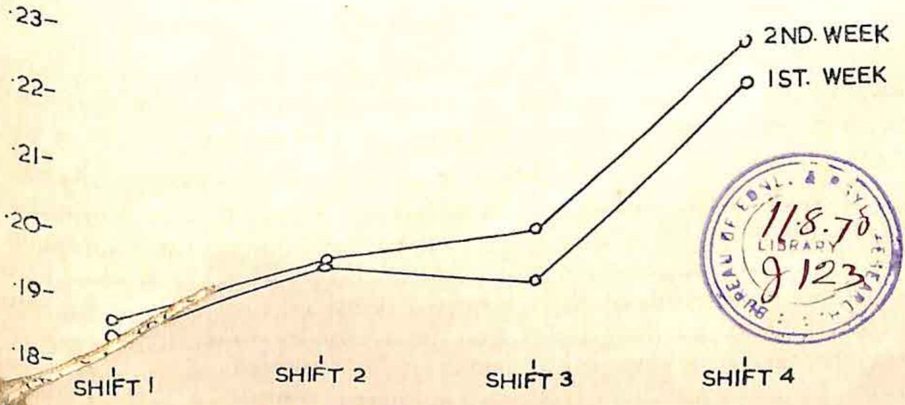

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FIG. 2b. MEAN COEFFICIENT OF VARIATION.

TABLE 2: PART I — MEAN OUTPUT IN JARS A MINUTE, SHOWING EFFECTS OF OBSERVATION

		WEEK 1	WEEK 2
(a)	Net	44.7	49.0
(b)	Observation periods	41.1	43.4
(c)	Remaining periods	41.6	41.8
(d)	Overall the day	41.4	42.6

PART II: PACED WORK (SINGLE)

Procedure

In the first part of the investigation values for the preferred unpaced packing rates were obtained for each operative. The object of the second part was to determine whether operatives could pack as many jars when they were being paced at the same rate. Inevitably the experiment had to be transferred to equipment which would deliver labelled jars automatically at prescribed and varying rates. Unfortunately the only machine that existed, had a minimum labelling rate which was nearly double the operatives' packing rate. But since in every other respect (not least an extremely accurate speed control) this machine was ideally suited, it was decided to test operatives working in pairs, one on either side of the belt on to which the machine delivered jars.

The layout therefore consisted of a number of operatives inspecting and feeding jars into an automatic labelling machine set at a controlled speed, this machine delivering jars on to a belt moving at 50 ft./min., on either side of which were two experimental operatives packing the jars by sixes into cartons. Further along the line were more operatives receiving these packed cartons, crating, labelling, etc.

Each packing operative worked on the experimental set-up for two half days, when possible on consecutive days. If on the first day she worked at this condition in the morning, she was tested in the afternoon on the second day, and *vice versa*. Furthermore for each of the two test periods the pairing was changed. This pairing was random and the design is shown in Table 3.

TABLE 3: PART II — PAIRINGS OF OPERATIVES AND MACHINE SPEEDS IN JARS A MINUTE

DAY	A.M.	P.M.
Monday	D A 91	
Tuesday		[illegible] B [illegible]04
Wednesday	B C 101	
Thursday		C E 95
Friday	E D 88	

The determination of the speed setting of the labelling machine involved some difficulties. The requirement was that the machine should deliver jars at a rate equal to that at which the two operatives under test packed when working unpaced. The first problem was to decide which of the four values in Table 2 to use. Clearly since close observation was to be made either value (a) or (b) would have to be used. But value (b) included stoppages outside the control of the operative, and there were no grounds for assuming that the proportion of involuntary stopped time would be the same in the two systems. On the other hand, value (a) was the packing rate whilst operatives were actually packing. It would be relatively easy to confine measurements in Part II to such periods.

The second problem was to decide whether the value obtained in Part I from either of the two test days was the more reliable. Every operative worked faster during the second day. Many guesses could be made at the reason why, but none sufficiently firmly based on evidence to be of interest. It was therefore decided to average the two rates obtained from each operative. The resultant machine speed settings in terms of jars/min. for the different pairs of operatives are also shown in Table 3. These speeds were set by the engineer first thing every morning.

Observation on each pair was made for approximately 100 working minutes, *i.e.*, when jars were being delivered at the prescribed rate. The criterion of performance was simply the number of jars which the operatives did not have time to pack. These passed on along the belt.

Results

During no period of observation did more than two or three jars in all pass by unpacked.

PART III: PACED WORK (TANDEM)

Procedure

The purpose of this condition was to compare the unpaced performance of operatives with their paced performance at the unpaced rate, when the work cycle was divided between two operatives. In the second condition each operative packed a complete carton of six jars. It had been the practice since this job was established to break down this cycle into three components, as so often happens in flow-line production systems. The most frequently cited reasons for this are that the breakdown simplifies each process, reduces training time, gives greater freedom of selection of personnel, and within the limits of the system permits greater mobility of labour. For this condition the work cycle was divided between two operatives.

The mechanical arrangements were identical to those of Part II. In this case though the two operatives worked side by side. The first one who was nearest to the labelling machine picked up an empty carton, placed in it five jars taken from the conveyor belt, and passed it along a greased track to the second. The second operative put in the sixth jar.

This apparently curious breakdown into five and one jars was in fact an equitable division of labour in terms of time and had been arrived at by the operatives through much trial and error long before the experiment began whenever they had worked in pairs rather than threes. It has been mentioned that the cartons were especially designed. The design had to meet certain requirements of the sales side of the company, and in particular the jars had to fit exceptionally tightly so as to avoid breakages in transport. There was ample space for the first five jars, but the sixth had to be 'worked' into the remaining space.

The pairing of operatives and the machine speed settings were exactly as in the previous condition. The remaining part of the day as shown in Table 3 was used to test this 'tandem' arrangement. This gave two work spells for this condition, and after the first spell the two operatives being tested changed positions.

Observation was made during each spell for approximately 60 minutes, the criterion of performance again being the number of jars which passed through unpacked.

Results

Table 4 shows the number of jars per minute which were not packed by each of the five pairs of operatives when each one of the pair was in the leading position. It is quite clear that when the work cycle was divided there was a considerable drop in output compared both with the paced condition when the work cycle was complete, and with the unpaced condition. The best operatives (in the unpaced condition), considered as pairs, not only missed fewer jars but also packed more, but nevertheless could not keep up with the machine delivering jars at a rate equal to their own joint unpaced rate.

TABLE 4: PART III — THE EFFECT OF PACING WHEN THE WORK CYCLE WAS DIVIDED

MACHINE SPEED IN JARS A MINUTE	OPERATIVE ORDER	JARS A MINUTE NOT PACKED
91	D : A A : D	18.4 11.7
94	A : B B : A	18.0 9.7
101	C : B B : C	8.2 3.8
95	C : E E : C	2.9 18.6
88	D : E E : D	14.8 17.9

The positions of the operatives were important. Large differences in output occurred when the same two operatives changed places. In most cases there was an advantage when the better of the two operatives led; [illegible] is because the second operative was paced as much by the first operative as by the automatic machine. If the first operative was slow, then the second would be deprived of opportunity to work.

Finally the consistency of the operatives' performance throughout the investigation is worth noting. No order changes showed to any extent. The relative output from the pairs in Part III could have been accurately predicted from that of the first week of testing. This largely reflects the high level of experience of the operatives, but probably to some extent as well a fairly consistent reaction to the experimental situation.

DISCUSSION

It is clear that a longer study might have been worth while. In Parts II and III operatives were under test for a relatively short time, though far longer than they would have been in a conventional laboratory experiment. In order to maintain the experimental set-up and two operatives at work, some fifteen persons were continuously required, to unload jars from trollies, to inspect them, to feed them into the labelling machine, to deal with whatever the experimental operatives missed, to handle the packed cartons, to manufacture the cartons used, etc. At the same time, the equipment which represented a large proportion of the total that the company had available for the packing process, produced considerably less than it normally would as a result of the way it was experimentally used. It was therefore essential to base results on this small sample of the operatives' performance. Because, however, of the high level of practice of the operatives, it is considered unlikely that substantially different results would have been obtained from more prolonged testing or from testing more operatives.

The main point of the results requiring discussion is the fact counter to the prediction made in the introduction that in Part II operatives performed as well when paced as they had done when working at their own free rate. Five factors seem to be relevant and they are listed in order, the least important first.

(a) The layout of equipment and method of working in Parts II and III was the normal. Although the operatives had all worked on the equipment used in Part I it was not normal for them to do so.

(b) In particular the topographical arrangements in Part I had not been so carefully thought out as had those in Parts II and III. For example, it was probably more uncomfortable for operatives to sit than to stand in Part I; empty cartons were not quite so conveniently sited; removal of full cartons was not quite so effective.

(c) Jars moving along on a belt can be comfortably picked up three at a time. If the hands are kept still in the right place, the jars will flow into them. With the Part I equipment it was easier for operatives to pick up jars one a time—but perhaps slower. It is no great problem to arrange for jars to be delivered in a line, but the necessary equipment was not available.

(d) In describing the Part II layout, it was mentioned that the labelling machine would not label at a rate sufficiently slow for one operative to attempt to pack its minimum output. Operatives therefore worked in pairs. With this arrangement it is probable that when one of the pair was in difficulties the other would spurt.

(e) It is possible to define when an operative is in 'difficulties', and an examination of the probable frequency of such occasions goes some of the way towards explaining the failure of the prediction.

The character of the distribution of operative cycle time when the work was unpaced is illustrated in Fig. 1. The first point of immediate interest is the range of values. This range is of significance because it shows the maximum amount of time that would be required to carry out one, or more than one, consecutive cycle. It also shows the frequency with which these maximum times would occur. The second point of interest is that a little over 60% of the cycle times are shorter than the mean time when the times of all operatives are considered together.

Now if most of the cycle times were of about the average value, operatives would tend to draw jars from about the same spatial position on the belt, since the delivery rate was fixed in relation to this average value. But with well over half of the values shorter than the mean, there would be a steady tendency for operatives to work ahead: *i.e.,* they would creep nearer and nearer to the limit of their comfortable reach in the direction of the jar source.

An operative tending to draw from a place directly opposite would always have available for a cycle her average time plus the time for a jar to pass from the directly opposite position to the point where she could no longer comfortably reach it. This latter time is a function of the linear speed of the belt, in the present case 50 ft./min. Assuming a reaching range of 5 ft., jars took 6 secs. to cross the total range. The available time in the above cited case would be the mean time of about 7·4 secs. plus 3 secs., a total of 10·4 secs.

But in fact, because of the tendency to work ahead implicit in the nature of the cycle time distributions, in most cases operatives would have 7·4 + 6 = 13·4 secs. to complete any cycle. Even if no other factors were present, considerably less than 1 jar per 100 would be missed, and it is highly likely that the other factors referred to above have reduced this 1% missed to insignificance in Part II.

It will be recognised that in this explanation, two factors which will vary from one conveyor belt situation to another, are of importance. These are the character of the cycle time distribution, and the conveyor belt speed. If either the range of times had been greater or the belt had moved faster there would have been a greater likelihood of jars being missed when the mean cycle time and jar delivery rate were unchanged. The belt speed is a physical factor which is, within very wide limits, entirely controllable. The range of cycle time values is less easily controlled and of immediate relevance is the increase in the value of the coefficient of variation during day. In a number of other studies (Fraser, 1952; Gugenheim, 1953) variability of performance has been associated with fatigue. The data reported here supports this view. If the association is genuine, then fatigue in operatives will affect the efficiency of these systems not necessarily by increasing the mean cycle time, but by increasing the scatter of times.

The result of Part III is that which was predicted. Since operatives were deprived of the artificial advantage described in point (d) above, which was considered important in the Part II results, the inefficiency in the system has been clearly demonstrated.

The first operative of the pair always had enough material (both jars and empty cartons) to preclude any waiting on her part. But unless the work of the pair was perfectly balanced delays would creep in. Thus if the first operative worked fast she would over-draw from the supply of jars. In this case the second operative would have a growing queue of partly filled cartons and insufficient jars to fill them. In time the first operative would have to stop, in which case far more jars than the second operative could deal with would flow down.

If on the other hand the first operative worked slowly, she under-drew from the supply of jars and failed to provide the second operative with sufficient partly filled cartons to deal with the surplus even if she could. Thus whenever there was imbalance (and imbalance is inherent in the system as shown by the cycle time distribution) jars tended to be missed.

CONCLUSIONS

In the development of conveyor belt systems the relationship between the psychological element represented by the distribution of operatives' cycle time and the mechanical element represented by the speed of movement of the belt is critical. The greater the range of cycle time values, the slower the belt would need to move in order to maintain a constant output when the mean cycle time and flow rate are constant. In any case, small coefficients of variation would give m[illegible] freedom [illegible] design which is usually linked with relatively fixed features such as [illegible] space, etc. In training operatives to man such systems it is evident that as much attention should be given to factors contributing to a basic distribution of cycle time as to a basic mean time.

On the other hand, many of the problems associated with balancing these lines can be obviated by the use of engineering systems which permit the establishment of controlled queues or stores of work at each processing point.

The type of system would depend on the nature of the material passing through it. The effect would be the elimination of both belt speed and cycle time distribution as factors determining output. Output would depend entirely on the mean cycle time, and the distribution would merely determine fluctuations in queue size. So long as the breakdown of work was into units of approximately equal time value, the average size of queue (and hence the amount of idle material) would tend to be equal and constant.

ACKNOWLEDGMENTS

The author wishes to express his deep appreciation of the generous co-operation given continuously by the directors, management and operatives of the Company concerned.

Dr N. H. Mackworth read the manuscript in draft form and made many valuable suggestions.

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The Efficiency of Labour in Coalmining

By W. H. SALES

Mr Sales is Member for Labour Relations on the National Coal Board. This is a verbatim report of a lecture he gave at the Board's Summer School at Oxford in September, 1954.

The spectacle of Benjamin Disraeli among his fellow members in the House of Commons was once picturesquely likened to the appearance of some foreign fowl in an English barnyard. The simile may or may not apply so aptly to me, here in Oxford, but as the new Member of the Board lecturing for the first time at the Summer School, I must confess to a feeling of strangeness.

Most of you will, I think, recall a film which appeared in this country in the days before World War II. The film was entitled, if I remember correctly, *A Yank at Oxford*. The "Yank" in that film was depicted as a chap who meant well but who blundered against the most sacred and therefore necessarily unwritten Oxford taboos. I hope there is little chance of my appearance at Oxford offending, unwittingly or otherwise, taboos ancient or modern. If, unfortunately, I do blunder against taboos, Oxford or National Coal Board, perhaps you will be kind enough to cast your minds back to the film I have referred to and at least regard me as a new chap who means well. Of course, you will appreciate that in talking for an hour or so about the efficiency of labour in coalmining I can scarcely avoid touching upon some shibboleths and on some controversial points. I know, too, that I cannot hope to impress you with a miracle of tight-rope walking, but I shall feel immensely gratified if I can match that extract from the pen-picture of an Oxford cleric by the poet Chaucer. More than five hundred years ago, in his Prologue to the Canterbury Tales, Chaucer wrote of him:

> "He never spoke more than was need,
> Formal at that, respectful in extreme,
> Short, to the point, and lofty in his theme."

I am told that this Summer School has for its general theme: "Now and Tomorrow", and a general lecture, I am advised, should have a theme which fits in with the general one. Very well, you shall have a theme. It is epitomised in the plea made by Robert Owen to his fellow employers over a hundred years ago: "If the care" said he, "which you can bestow upon machinery can give you such excellent results, may you not expect equally good results from care spent on human beings, with their infinitely superior structure?"

For those of you who prefer to hear a more recent soothsayer, what Henry Ford II, son of the foremost practitioner of modern production techniques, told American industrialists will do equally well: "If", he said, "the problems of human relations in industrial production can be solved, as much progress towards lower cost can be made in the next ten years as was made during the past quarter of a century through the development of the machinery of mass production."

My precursors on this platform have doubtless expounded some of the customary tests of industrial efficiency. You will probably have gathered that efficiency is like the many-headed Hydra of classical mythology—no sooner is one head cut off than another two grow in its place. The particular aspect of efficiency which falls to my lot, today, concerns the fillers, the packers, the haulage hands, etc.—in fact all the human beings who have jobs to do in this great coalmining industry. For purposes of identification I have given it the name "Labour Efficiency". In basic English this could be translated as "how well or not well they work".

Now in any industry how well or how indifferently men work is important; but in coalmining it assumes a pre-eminence not to be found elsewhere. The reason for this is that mining is an industry in which the cost of labour in relation to the total cost of producing a ton of coal is extremely high. One of the few generalisations about coalmining which stands up to scrutiny is that the cost of labour as a percentage of total costs varies only within narrow limits. At practically every colliery in this country, the proportion of wages cost to the total cost per ton of coal mined is in the region of 60 per cent. This is therefore a typical and representative figure, true not only of one or two collieries but of virtually all. In respect of this general feature of a high ratio of labour costs, coalmining stands out on its own like a New York skyscraper would stand out among the buildings of our towns and cities. Those of you who have been to Blackpool or to Paris will be able to visualise the way in which the famous towers of those places dominate the other buildings. Coalmining is just like those towers, because in respect of this characteristic of high labour costs it is well out of the general run of industry. In no other major industry is this percentage so high: in Heavy Chemicals and Allied Trades it is about 10 per cent.; in the Food, Drink and Tobacco Manufacturing industries it is 5 per cent.; in Gas, Electricity and Water Supply and in Metal Manufacturing it is about 16 per cent.; in Textiles 14 per cent. Even in Engineering and Shipbuilding it is only 25 per cent., jumping to 35 per cent. in the Building Industry. The cost for all industries is only about 20 per cent.; labour costs in coalmining are therefore three times as high. Incidentally, this has an important implication: it means quite simply that almost every national wage increase, unless accompanied by an increase in productivity, involves a *substantial* increase in cost. In those industries where labour costs are only a relatively small proportion of total costs there is more room for manoeuvre in making economies which enables them to absorb an increase in wages with relative ease.

To my mind, this alone is ample reason for concentrating the utmost attention and effort on raising the general efficiency of the labour force. This, in turn, entails much closer study of the conditions under which this may be achieved. Although this high rate of labour costs is probably the most important *single* economic fact of coalmining, and is familiar enough to a good many people, yet I question whether its implications are ev today fully realised. Or if they are, then they do not seem to be acted upon.

Let me give you one general illustration. The greatest pains are frequently taken to collect and analyse operational data on machines in action; but only relatively meagre operational data has been collected on the behaviour of the human beings who work and look after them. Infinite pains and concentrated thought are given to the planning of some technical feature. Is there so much thought given to ensuring that the conditions influencing "labour efficiency", in contradistinction to technical efficiency, can be improved? In short, is there sufficient thought on how to induce or create the conditions under which men at work will give of their best? I grant you that the interpretation and manipulation of the one is more subtle and intrinsically difficult than the other. But is difficulty alone the reason?

Is it because the problem bristles with too many difficulties, which makes "labour efficiency" such a Cinderella, and is this why it does not receive that attention for which this basic economic fact of high labour costs shrieks, so that we escape from it by the modern industrial equivalent of the Nelsonian trick of turning a blind eye to anything we do not wish to see?

Or is it because the deep shadows of the past hang so heavily over us that we cannot see clearly through the gloom? Mr E. H. Browne in his speech on "The Management of Collieries" remarked that out of 310 papers presented to the National Association of Colliery Managers over the last eight years, 180 were concerned with mining methods and machinery, 20 with health and two with accidents, 14 with human relations, and four with the organisational aspect of colliery management. And this is an industry whose labour costs represent two-thirds of the total costs!

These statistics, I think, illustrate as well as any my contention that the fundamentals of "labour efficiency" do not seem to have attracted the attention they deserve; the bias has plainly been on the technical side. I would be the last person in the world to minimise the importance of technical efficiency or denigrate the work of technicians, but it seems to me sheer shortsightedness to concentrate on technical methods and innovations alone. We have, I am sure, accumulated enough evidence to show that new and better machinery is not enough. Yet it is still true that economists are not the only people who have taken a too mechanistic view of the behaviour of people at work, and it is this view which must be shaken off, because in the handling of men there is no room for indifference or defeatism. A great deal more thought and introspection, observation and experiment is required in the coalmining industry on this subject.

So much by the way of introduction. At this point I should like to stress that the term "inefficiency" when applied to the men employed in this industry is apt to carry with it aspersive overtones, and to some may appear derogatory or condemnatory. I would therefore ask you to purge your minds of any such moral implications which the word may connote. I shall try to use the term in the way an engineer applies it to a machine. When he says that the thermal efficiency of a coal-fired express railway locomotive is less than 10 per cent. or that of a petrol-driven motor car is about 25 per cent., he means that only 10 or 25 per cent. of the full potential of th energy locked up in the coal or petrol is obtained in practice. The differenc between the theoretical and actual work value of the heat content i expressed as a percentage under the name of thermal efficiency. When I tal about labour inefficiency, no more is implied than if an engineer were t talk about the thermal inefficiency of the latest locomotive or of the fines car: it represents no more than the gap between possible capacity an attained performance.

Having cleared this, let me now broach the subject of labour efficienc from the negative angle. Some of you may have noted that I have n defined what I mean by "labour efficiency". Apart from stating categori ally that it is not synonymous with output per manshift, per man week c per man year—though it is sometimes mistakenly used as such (although is an ingredient of o.m.s. in much the same way as flour is an ingredien of cake)—I doubt whether I can do so in meaningful quantitative term The customary industrial tests of labour efficiency are in reality misnomer for strictly they are measurements of the inefficiency of labour at work. Th chief measures of labour inefficiency available in coalmining are figures on (i) Restrictions and Strikes; (ii) Absence; (iii) Accidents; and (iv) Labou Turnover.

I am well aware of the snags and the defects of statistics; the mere fac that they are not more precise than they are is but another reflection of th lack of attention hitherto devoted to problems of measuring and evaluatin the behaviour of men at work. But this is not a talk on statistics; all I wa to do here is to mention that if we estimate the inefficiency of labour i terms of shifts lost, then we can gain a rough idea of what was lost in 195 On the basis of time lost, which is by no means the whole story, we fin that if we share out the total time lost over the whole labour force, includin those who did and those who did not lose any time, then we find that:

0.1 shifts were lost per man/year through breakdowns, etc.
0.6 shifts were lost per man/year in disputes;
13.9 shifts were lost per man/year through voluntary absence;
20.7 shifts were lost per man/year through involuntary absence;

making a total of 35.3 shifts lost per man/year, or the equivalent of seve normal five-day working weeks. These figures exclude all holidays, whi in 1953 were the equivalent of just over three normal working weeks. Th nearly 15 per cent. of the working year was lost in these various wa What this means in terms of output is anybody's guess; but would anyo put it lower than five million tons, or less than 2½ per cent. of the 19 output of saleable coal? Some no doubt would be prepared to put it a go deal higher, but my purpose today is not to ask for bids but to draw att tion to the dimensions of the waste and inefficiency on the one hand, a on the other hand to the potential gain that would result if it could reduced. Here, for all to see, is a fruitful field for attack—and a challen to everyone in this industry. Here is observable and measurable evi on some aspects of how men actually behave in this industry of ours. M could be gained by the reduction of these aspects of inefficiency of labour. Yet I have seen clever men, able men, sincere men, who delight to battle with the subterranean natural forces in our coalmines, men who ardently pour their ingenuity into devising a new machine or into improving an old one, men who will devotedly spend hours concocting some new technical feature, or burn the midnight oil seeking the answer to a practical problem in mining engineering, men who almost worship the god of technical efficiency and to whom inefficiency in its technical aspects is anathema; yes, I have seen those self-same men quite inured to accepting as immutable the heavy burden of labour inefficiency. And furthermore, whereas they would no more dream of introducing outmoded machinery into coalmines, or of planning their layouts in accordance with the "best practice" of fifty years ago, they will swallow uncritically and apply without question the outworn and defective maxims of the nineteenth century. Show them a technical problem and they are all agog; propound an admittedly more difficult "labour" problem and interest fades, shoulders are shrugged or the light of defeatism shows in their eyes.

I am, of course, deliberately presenting the case with emphasis. Not to do so would lessen its effect. I am sure this emphasis will not be misunderstood. I should be sorry if it were because I should not dissent from anyone who drew my attention to the positive actions taken by the National Coal Board and by Management up and down the country to deal with the problems I have just presented. My chief concern is to point to the tendency of old ways of thinking to survive when techniques and institutions have moved forward and to emphasise the ever-present danger of every one of us slipping back unless we deliberately make it a daily act of faith not to do so.

With this warning perhaps I can now say, without danger of being misunderstood, that if we show indifference to the behaviour of men at work it can only mean that we have abandoned all hope of any spectacular improvement on the labour front. That we have given up any idea of preventing strikes, or of reducing absenteeism or of cutting down accidents (except by technical means). That we have given up workmen as incorrigible and hopeless, as a shopkeeper might reluctantly write off bad debts. Does anyone really believe this? I think the answer would be "no", but I am sure there is a kind of hypnosis which undertones the "no" and this hypnosis is not confined to levels or departments. It lurks in the carefree atmosphere of Hobart House and in the Divisional and Area Offices; it is to be found at collieries, on the surface and underground.

One of the assumptions of the National Plan is that practically no improvement is expected in the efficiency of labour. It is only fair to say that no more should be read into this than into any assumption made about variables. It is probably true to say that no account was taken also of certain probable increments in technical efficiency. But whereas in many people's hearts they expect technical efficiency to improve, and not one of them would refuse to bet that it won't—their only difficulty being to say by how much—I sincerely doubt whether any one of them would be as certain about an improvement in labour efficiency as measured by a drop in strikes, absence, accidents or labour turnover. In other words, they would be unpleasantly surprised, dismayed and disappointed if the planning assumption about improvements due to technical innovations were not falsified, but would be pleasantly surprised if the planning assumptions about labour efficiency were to any degree confounded by events. I hope it is not necessary for me to say here that I am not quibbling about the merits or damning the assumptions of the National Plan; my object is simply to illustrate the extent of the hypnosis and to suggest that it places those who succumb to it in the dilemma of concentrating on technical advancement as the only hope of raising production and productivity with at the same time the fear that the potential improvement from technical efficiency may be nullified by inefficient labour.

Are we in a groove, taking a worm's eye view of human nature at work and accepting the dictum that it is "solitary, poor, nasty, brutish and short"? Some call that being realistic, and if they do it can only mean concentration on technical blue-prints for improving productivity without so much as a thought to the impact of the plans on the behaviour of the men who will carry them out. It is no doubt very alluring (it is to me as well as to the technician) to dream of a press-button coalmine running with the smoothness, orderliness and industry of an anthill. But we are a long way from the technological millennium of coalmines where only the technical habits of machines need to be taken into account. Meanwhile we have to deal with coalmines where the social habits of groups of men are paramount; if we ignore this, the gap between expectation and realisation may be disappointingly wide.

I now turn from the negative to the positive aspects of labour efficiency. So far I have confined my attention to better known, visible aspects of labour inefficiency. But these blemishes are far from representing the whole story. The gap between measurable capacity and performance in any industry is great. But this visible gap represents only part of the waste due to failure to develop goodwill and capacity for collaboration. If we compare the strike and absence records of the best and worst pits, some truly remarkable differences come to light. The best pits have had no losses due to strikes for twenty years or more; but one pit had no less than 171 strikes in the years 1949-1952. Similar differences are revealed in the rates of absence; and these show up even between Divisions. Studies of accident rates also show several significant differences between collieries, differences which are not entirely accounted for by variations in physical environment. And if, as I suspect, these only reveal part of the waste, what of the untapped potential we have not attempted to measure? What of the losses, no less important because they have so far resisted measurement, attributable to the shortcomings of the techniques in current use because of a failure to enlist the full and willing collaboration of the men and women who work in this industry? The losses and waste we can detect are probably no more representative of the true loss than the visible portion of a floating iceberg is of its real dime sions. We can no more define the extent of the untapped potential than we could tell the extent of a coalfield or an oilfield by sinking one or two boreholes. But we have more than a hunch that it is waiting to be tapped; the sensible approach therefore appears to be to set about tapping this potential with all the verve, skill and determination that is brought to bear on refurbishing the technical side of the industry.

Last year, Sir Andrew Bryan drew attention to the remarkable variation in productivity—that is, o.m.s. overall—between the Board's 900-odd coalmines. He suggested that only 60 per cent. of this variation could be accounted for by natural differences between collieries, and concluded that the balance of 40 per cent. if not due to nature must be due to nurture; and being man-made could be man-corrected. Calling the missing factor X, he urged the need to track down the cause of this man-made variation, which, as he pointed out, probably varied in value from pit to pit. I am not going to presume that all this 40 per cent. of the man-made variation is attributable to variations in labour efficiency; some of it will be due to varying degrees of organisational efficiency. But I should be surprised if some of the missing X was not eventually tracked down to real differences in the performance and behaviour of men at work.

Sir Andrew Bryan in this talk concentrated his attention on the missing X; in effect, he was saying that if we could bring the worst pits to the standard of the best then, irrespective of geological conditions, the gap between the worst and the best pits in terms of productivity would be nearly halved. And the important point to note is that he was talking in terms of actual and therefore known standards — standards as concrete as the stone Dr Johnson is reputed to have kicked in order to refute Bishop Berkeley's philosophical ideas about reality. But if the gap between capacity and performance is as great as many believe it to be, if the gap between what has been achieved at certain collieries and others is demonstrably great (judged merely by the crudest measurements of labour efficiency), what of the untapped potential? Whatever the measurable dimensions of Sir Andrew's missing X may be, what of the unmeasured missing "we"—that untapped potential. If only we could find a way of concentrating our skill and attention on the men, as much or more than we do on the machines and other technicalities, I feel certain that the efficiency of labour, and one of its results —higher productivity—would show one of those remarkable jumps signalled by industrial progress whenever a new technique has been successfully applied.

Is this industry—an industry employing men who worked so hard for it to be nationalised—is it ripe for one of those periodic epoch-making jumps? I do not know. It would be rash to prophesy, but of this I do feel certain —that if this jump is to be, then it will stem not only from technical innovations but from new techniques of influencing men to give of their best and to find in work the full satisfaction for the creative urge which is in all of us. Some men, more perspicacious than I, claim to discern four phases in the development of modern industry:

The first they call the materials phase; it was concerned mainly with the discovery of new industrial processes.

The second concentrated more on the invention of new plant.

The third phase—the methods phase—lasted up to the end of World War II and concentrated mainly on the organisation necessary to facilitate large-scale production.

The fourth phase, labelled "the manpower phase", which is only now beginning, will, they think, be concerned mainly with the extraction of the maximum human effort.

If this analysis is correct, then we are now in the so-called manpower phase when we must learn how to handle the human element and how to develop human power. Any spectacular improvement will depend a great deal on our success in this direction. One need not, of course, accept the historical assessment just mentioned. But whether one accepts it or throws it out of the window, the validity of my thesis is unaffected; for it is based partly on verifiable fact and partly on a rational belief that the capacity for co-operating in the productive process is unlimited. Furthermore, our attempts to do so hitherto have been crude, sketchy, lopsided and uncoordinated; not even as advanced as the social skills demanded of and exhibited by men who co-operate in the playing of team games or team activities.

The central problem in all this—if I may now turn from historical analysis to work analysis—is how to persuade (since there is really no satisfactory means of forcing) men to co-operate fully and so give of their best at work. Hard work, not necessarily the same as physically hard work like building pyramids or the Great Wall of China, means intense application, concentration and sticking power; it is invariably the product of the worker's own desire, and the result of his own personal intention. Given this, no more is needed than to provide the worker with the opportunity to "get cracking". But if the will or the zeal is not there, if we cannot take that for granted, then supplying the worker with the opportunity alone is no guarantee of results. Both must come together, the one matching the other, for first-rate efficiency to be attained. Obviously the task of converting a reluctant or semi-reluctant into a ready worker is by no means easy or straightforward. The quality and amount of labour offered by men to dispose of in the technical process of production depends on the worker and his morale. How much he offers and how much he withholds in turn depends on a large number of factors, about which, let me confess, not a great deal is known.

"The striker is unloved, unhonoured and unsung; above all, he is largely unstudied", said one recent writer in his monograph on strikes. That could be adapted to apply to "the worker at work" without forcing the facts. It is certainly beyond all dispute that he is largely unstudied. I do not count as serious study the plethora of half-baked maxims and half-truths that men cull from their personal, limited experience. For one thing, personal prejudice and bias are apt to creep into such judgments; for other, the interpretations of one will be opposite in polarity to those of another; and finally, they are no more than by-products of so-called "personal experience" which is no substitute for the deliberate and objective study of the worker at work. In other words, they do not derive from scientific investigation and experiment, but from the crude processes of trial and error often but crudely analysed and passed on by word of mouth. Many of these maxims are not far removed from myth — industrial myths, if you like — but possessing the power to mesmerise in the same way as other twentieth century myths have been known to cast their potent spell over whole nations. The time has arrived when we should no longer be satisfied with the accumulated wisdom of personal experience alone. Facts are needed and demanded; not facts derived from prejudices, whims and personal fancies, but facts based on objective methods of observation, measurement and experiment.

Most of us still think about and act towards men at work as if they were individuals subject to no pressures or controls other than their personal desires, whims or fancies. A commonly accepted assumption is that the worker, if he is to be studied at all, must be studied as an isolated unit; and that in certain important respects he resembles a machine whose efficiency can be measured or estimated. The main factors influencing his efficiency at work are thought to be:

(i) wasteful or ineffectual movements in doing his job;
(ii) fatigue;
(iii) defects in the physical environment such as poor lighting, ventilation, excessive humidity, and so on.

Whilst there are no doubt circumstances where the individual may be studied with profit and where attention to all these matters helps and often succeeds in improving efficiency, they are nevertheless far from telling the whole story. More than 25 years have now passed since experiments were carried out by Elton Mayo at the Hawthorne Works of the General Electric Company in Chicago in an attempt to improve efficiency by improving physical conditions. (He was not, be it noted, primarily concerned with industrial efficiency as a social problem.) It is claimed that the results of those experiments, repeated elsewhere over and over again, showed the unexpected and, at that time, inexplicable result, that no matter how the physical conditions were varied, there had been struck a factor which increases output no matter what was done. I have no time to give you the details, but he and others since claim that it is utterly mistaken to think of, say, relations between management and workers as relations between officials on the one hand and a number of separate individuals on the other. The relations are not related to single workers, but always to working groups. These groups may vary in their size and cohesion, but they exist as groups nevertheless.

Further, it is claimed that working groups exercise a potent influence, for good or bad, over the behaviour of their individual members. In particular with the amount of work they accomplish, their absenteeism and accident rates, their propensity to strike, even their rate of turnover. In short, the working group influences precisely those factors which are basic to the efficiency of labour. It is suggested that the influence of the group is the greatest single factor in go-slows, ca'-cannys and other phenomena typical of restriction of effort at work. Persons with no social anchor, so to speak, tend to have no feelings of loyalty and to take little interest in what goes on around them.

If there is anything in these claims, then the key to improved labour efficiency, and through improved labour efficiency to higher productivity, and to the full utilisation of the technical means of production, is without question in the working groups which go to make "men on books" at our collieries. The problem of absenteeism, labour turnover, low morale and poor efficiency reduces to the problem of how working groups may be solidified and collaboration increased in the large as well as the small colliery. And by working groups is not meant all the men on a coalface, on a haulage plane, in the workshops, or all the men in a particular district or on a given shift. What is meant are the small informal groups of about 10 to 15 men, rarely more but often less, which coalesce from larger numbers in much the same way as raindrops coalesce from a cloud of water vapour. Engineers, physicists and biologists are familiar with dozens of different units—from a unit of electricity to a unit of a metal or other chemical substance, or to the biologists' unit—the cell. For anyone concerned with the behaviour of men at work the primary or natural unit appears to be the small working group; the basic unit both of contact and of industrial research, exercising strong influence at a colliery over the work habits and attitudes of individual workers. If this is so, then to create conditions favourable to the development of such working groups by, for example, not switching men too frequently from one group to another and in so far as is possible arranging work in terms of such groups, should be paramount considerations. What will suit human nature best should be the first question which anyone concerned with organising the productive process should ask; furthermore, it should not lightly be allowed to play second fiddle to arrangements preferable perhaps on technical grounds. The building up of strong closely knit working groups and of obtaining their collaboration is not something which occurs by accident — at least not often. It must be planned for and developed with as much care and assiduity as a gardener bestows on his prize blooms.

I cannot over-emphasize that a coalmine has two major functions to perform: the economic one of producing coal and the "social" function of creating and distributing satisfactions among those who work there. A good deal of time and energy have been devoted to the production function, but has enough been given to the "social" function? It is evident that if the "social" organisation of a colliery is primitive or out of balance, all the efficiency systems and the financial incentives imaginable will have little effect on output and productivity.

This brings me to the question of Work Study as a means of improving the efficiency of the labour force. Here is a matter about which we shall probably hear a good deal more in the future than we have in the past. A colleague of mine defined it as "organised commonsense applied to practical jobs that men do in this life on earth". I would like you to note this definition very carefully, for mention of the words Work Study is apt to generate so much white-hot emotion as to exclude all rational thought about it. Whatever we may think about its past exponents or for the systems that have masqueraded under this name in the past, no one will, I think, deny the crying need for close, accurate and objective observation and measurement of what goes on and how things are done by men in their everyday work in the collieries. For the study of things as they are is the first essential step towards substituting something better. But—and this is the rub—it all depends, as he late Dr C. E. M. Joad of Brains Trust fame used to say, on what you mean by observation. The danger is that only certain selected aspects of the total situation—the tangible physical things which can be visually observed and are therefore more susceptible to measurement—will be abstracted for study. And these will give us only part of the story—just as a black and white photograph of a landscape fails to reproduce the important attributes of depth and colour. If those whose job it is to develop and apply work study techniques limit their observations to the equivalent of obtaining only black and white photographs, the conclusions which they will draw from such emasculated data, being incomplete and partial, may well be false, and, if not false, not as effective as they could be. My fear is that they will concentrate only on those physical aspects of work which are easily measurable in conventional units and ignore the less tangible, more subtle, non-physical yet infinitely more potent aspects of factors which influence the will to work.

Yet the efficiency of the labour force, of those groups of men who perform in diverse ways the work of extracting coal from the earth at 900-odd Board-owned collieries depends as much on the non-physical factors which, I fear, may be ignored or overlooked, as on the physical factors upon which observation tends to be lavished. Since the pioneers of work study started their investigations almost three-quarters of a century ago, a good deal—though by no means enough—has been discovered about the second class of non-physical factors and the way in which they influence the will to work and hence the efficiency of the labour force.

It is true of course that the problems of human motivation may be more profound and complex than has yet been recognised, but it is clear beyond any shadow of a doubt that the incentive to work and the reasons why men work efficiently are the net result of a *large* number of interacting factors—for example, none of the researches into motivation substantiates in the slightest that the worker is only motivated by financial gain—and surely the large number of factors, positive and negative, internal and external to the mine merely underline the fact that workers are human beings. Who, for example, would dispute that each one of us has very complex motives for doing this or that. One may be predominant, but no single motive can ever be isolated as causing a line of behaviour. A typical group of 200 people will mention between them some hundreds of different reasons on their motives for work, or for absenting themselves from work. Sub-division may be useful for analysis and discussion so long as the total situation is not neglected in favour of the partial one. This is only another way of saying that the worker is a human being and acts like one even at work. Moreover, he is a social being and merges his identity with other workers, tending to evolve common or group attitudes and practices. These group attitudes and practices are thought to have the most important *single* effect—I repeat, *single* effect—on output and everything so far points to the conclusion that the approach to the understanding of the worker must be by the study of *men* at work.

Consequently, unless work study embraces this second class of non-physical factors which are known to have a profound effect on the way in which groups of men actually perform in terms of pure technical criteria, I do not see how it can hope to show us how fully to improve the efficiency of men and through them to use to the best advantage the machines and other contrivances they use at work. In short, Work Study which ignores the worker or treats him as a mere cypher can never be more than an indifferent tool.

Nobody is likely to stimulate anyone else to work unless he knows how that person is motivated and to what incentives he will respond. Yet it seems to me that confusion seems to be greatest on the very points we ought to know most about. It is still generally held, for example, that most people work *only* because they have to in order to keep body and soul together; that money is the only positive incentive; that compensations such as more "welfare" keep workers happy or mitigate their natural dislike of work; or that unemployment drives a man to work harder through fear of losing his job. In short, they still cling to the carrot-stick-bribe hypothesis of motivation, even though its shortcomings are obvious and widespread. Others think that the worker will work harder or more efficiently if we take pains to improve the physical conditions under which he works—temperature, noise, humidity, ventilation and illumination; others think time and motion study by eliminating inefficient movements will also accomplish a good deal in this way. Still others think that it is only necessary to provide him with the physical tools or other physical aids. Whilst others accept the worker for what he is supposed to be—more or less a lazy good-for-nothing dolt—and place their bets or their shirts on bigger and better machines and ever larger and more costly doses of more advanced technology. Now some of these are known to be doubtful starters and others "also rans" in the race for better labour efficiency.

I know it is difficult to define exactly what is meant by work, and how it differs from play or from leisure is one best suited for discussion by Oxford dons over their port, or whatever they drink nowadays. It is as hard to draw a distinction between work on the one hand and play or leisure on the other as it is to draw a line of demarcation between the spheres of interest of Trades Unions. But it suffices for my purpose if I denote what functio work performs in this industry. I have suggested there are two functions—the economic one of producing coal and the social one of creating and distributing satisfactions. Work activity produces fellowship and social contacts, and I seriously suggest now that the non-material social rewards such as respect, admiration, esteem and fellowship are rated so highly by men that we are forced to the conclusion that any industry which ignores this powerful weapon is neglecting a master key to the puzzle of obtaining higher efficiency. The solution is not so much as is generally supposed to find means of bribing, threatening or tempting unwilling workers into doing a job with a will, as of creating the situations and conditions which permit men to work in a way that will satisfy both their material and social needs, and so develop high morale. Failure to do this will inevitably turn work into "a source of strife, resentment and boredom".

I am not saying that money does not matter; of course it does. But that is not the same as thinking that it is the only thing that matters. For workers are not governed solely by economic motives. For a failure to recognise this in practice and for neglect of the "social" as opposed to the economic function of work a high price is being paid in the inefficiency of labour—in strikes, restricted output, absence, accidents and turnover, in what has been aptly described as a "vast sea of human waste". The traditional nostrums do not in themselves produce high morale, and we must clearly look beyond and behind them if we are to get the best out of men at work.

My conclusion therefore is this: we have, in my view, been ignoring some fundamentals; we have concentrated our attention mainly on the surface symptoms. If human beings at work stubbornly persist in being human, if our propensity to work is whimsical and if our productivity is inadequate in relation to an acknowledged need, this is as much a "social" problem as a technical one. The technical means to greater efficiency and productivity, the services we can provide by good administration and good organisation are not in themselves enough; the will, the desire, the determination to use these tools and services must also be stimulated. Unless the will and desire to see the virtue, indeed the necessity, of producing more and still more is present amongst those who arduously and courageously tear out the coal from the bowels of the earth, it is as certain as I am standing here that not enough will be produced. Our primary job is to study the worker at work; we should eschew the attitude that the *only* hope lies in concentrating attention on technological improvement, and make a new all-out effort to discover and apply the means whereby human capacity to collaborate in the productive processes basically necessary to our standard of life can be enlisted, not in part, but in full. Nationalisation, as we have discovered, does not of itself solve this problem. It may create more favourable conditions in which solutions may be pursued, but it does not provide the solutions. My plea is therefore that we should vigorously pursue these solutions, not on the basis of inadequate and outworn ideas about economic incentives alone, but through the application of what Elton Mayo called "social skills".

May I quote from a lecture I gave to Deputies some two or three months ago: "There is", I said, "a real need to look behind the scenes and to look very hard at the conditions under which men work. I do not only mean we should look at the physical conditions such as heat, water or dust, but at the way their work is organised in the broad sense of this term. For example, is it organised in such a way as to give the workman status, self respect, satisfying social relationships with other workmen and with his supervisors, the deputies, appreciation of his efforts, realisation by others, as well as himself, of his contribution; is his work routine steady or constantly broken; are his friends always being shifted around; does his pay go up or down for reasons he fails to understand? Are proposed changes explained to him and reasons given, or does he feel constantly at the mercy of technicians? Is the deputy the guardian of his status as well as his safety?" These are the things that matter to a man as well as money.

May I also quote from an address I gave to Colliery Managers in Lancashire some three years ago, employing a nursery rhyme to illustrate my text:

"For want of a nail, a shoe was lost;
For want of a shoe, a horse was lost;
For want of a horse, a rider was lost;
For want of a rider, a battle was lost;
For want of a battle, a kingdom was lost;
And all for the want of a horse-shoe nail."

There are many nails lying hidden and unused in the field of *men* management.

I use these quotations only to show that in the past, as today, I have tried to expound a theory of work fitted to the present time, a theory of work which treats production and productivity as a human question, not just as a technical question. It denies the corrupting theory which assumes work is a natural evil. It postulates that investment in human capital can be as rewarding as investment in technical capital. It stresses that the men who invest their *lives* in this industry are as worthy of study as the machines which are invested. It claims that if we want the worker to give of his best then work must be made more than economically satisfying. Historians tell us that one of the great triumphs of medieval Christendom was its success in making work socially satisfying. I am not sufficiently competent as a historian to say whether or not that was the achievement of medieval Christendom, but if it is true then we must try to repeat this success in the infinitely more difficult industrial conditions brought about by machines and machine technology.

Let us have done with regarding men as cyphers or economic wraiths, and study and develop the conditions which will enable them to attain those satisfactions which working with their fellows can confer. This boils down to what was so succinctly expressed by Alexander Pope in the words "*the* proper study of mankind is man".

The Ishihara Test and Defects of Colour Vision

By PETER CAVANAGH

The original of this memorandum was written for the guidance of psychologists working in the Industrial Rehabilitation Units of the Ministry of Labour and National Service. The author is indebted to the Ministry for suggesting that it should be given wider circulation. For considerable help in the preparation of this revision he is very grateful to Dr R. W. Pickford, Head of the Department of Psychology in the University of Glasgow.

THE INCIDENCE OF DEFECTIVE COLOUR VISION

DEFECTIVE colour vision, particularly among men, is by no means uncommon. In the early twenties estimates were generally about 3 to 4 per cent. in the case of men. For example, in 1924 Parsons wrote that 'in its grosser forms among civilised races it is said to affect about 4 per cent. of the male population and 0.4 per cent. of the female; but the tests from which these statistics are derived were often crude'. In 1925 there was distributed in the United States Ishihara's *Tests for Colour Blindness,* first produced in Tokyo in 1917. The new test had a very favourable reception and, as Murray (1948) has remarked, 'for a dozen years it was widely used in mass surveys, often with quite naive and uncritical alterations of test-procedure, scoring and interpretation; and some thoughtless printing of keys and plates in popular literature. While this invalidated the comparison of statistics, and examiners were further misled by a badly Englished introduction, one fact emerged unchallenged. Though no provision was made, as in the Stilling (Pseudo-Isochromatic Plates) for the detection of blue-yellow blindness, the test screened out almost twice as many colour deviates as the old standby, the Holmgren wool-test'.

In 1937 Collins wrote that 'the percentages given (in some of the more recent investigations) seem to be much higher than in the reports of the earlier investigations. It does not necessarily follow that the incidence of the defect is increasing; the indications are rather that detection is more accurate owing to the improvement of the test material'. The results of researches by Waaler (1927) and von Planta (1928) illustrate this point. Waaler, who tested 9,000 Norwegian schoolboys and a similar number of schoolgirls, found 8.01 per cent. of the boys with defects of colour vision and 0.44 per cent. of the girls. Von Planta, who tested 2,000 German boys and 2,000 girls, found that 7.98 per cent. of the boys had defective colour vision, compared with 0.43 per cent. Miles and Craig (1931), who used the Ishihara test, reported an incidence of 8.4 per cent. among male American university students and 7.2 per cent. among salesmen. In this country, Vernon and Straker (1943) reported that of some 55,000 to 60,000 war-time candidates for the Royal Navy who had been tested with a selection of the

American Optical Company's *Pseudo-Isochromatic Plates* (1940 edition), 7·45 per cent. had defective colour vision. Grieve (1946) found that, of 16,180 men, Royal Air Force aircrew candidates, who had been tested by one person using the Ishihara plates, 6.63 per cent. had defective colour vision.

Brown (1950), who examined 525 Scottish schoolboys and 252 schoolgirls with the Ishihara test, found that 7·43 per cent. of the boys and 0.46 per cent. of the girls were colour defective. In a further investigation Brown found that there was no significant difference in the colour vision of school children and adults. This is in keeping with the work of Chapanis (1950), who used five pseudo-isochromatic tests, including the ninth edition of the Ishihara test and the 1940 edition of the American Optical Company's test, in an investigation of 574 males and females, aged from seven to seventy-seven. He found a slight positive relationship between age and colour vision test score, but concluded that this relationship is due almost entirely to the poorer scores earned by those under fifteen years of age, and not to a higher incidence of defects of colour vision among the young. He found no evidence that very old subjects are handicapped on tests of this sort.

There is a certain amount of evidence that the incidence of defect varies with racial and cultural patterns. Clements (1930), using the Ishihara test, found that only 1.92 per cent. of a group of 624 American Indians and 3·71 per cent. of a group of American Negroes were red-green blind. However, among a group of 320 Todas, a tribe in southern India, he found an incidence of 12.80 per cent. Geddes (1946) reports an incidence of 0.5 per cent. of red-green blindness among 387 adult Fijian males, and Simon (1951), who examined 537 male Bagandas of Uganda, one of 1.86 per cent. Both these investigators used the Ishihara plates. The variety of tests in use—Hardy (1951) has written that 'more than 200 devices or methods for testing or measuring colour vision have been proposed, described, or used' —the crudeness of many of them, and the frequently unsatisfactory way in which they are administered, make it difficult to get a clear picture of racial differences. However, Vernon and Straker have shown a significantly higher incidence of colour vision defect in the western than in the eastern part of Britain. The following table gives the percentage (corrected for intelligence) of defectives in nine areas of Britain, a number of the main centres in each area being shown. Each percentage is based on more than 6,000 cases, examined with a shortened version of the American Optical Company's test.

East Scotland	Wick, Edinburgh, Galashiels	*5·00*
East Coast	Newcastle to East Anglia	*6.65*
London and South-East England	Chatham, Aldershot, Brighton	*6.69*
West Scotland and North-west England	Glasgow, Ayr, Carlisle, Barrow	*7·31*
Industrial North-central	Chesterfield, Derby, Nottingham, Leeds, Northampton	*7·3*

Industrial North-west	Stoke, Liverpool, Manchester, Wrexham	7·41
South Wales and Somerset	Cardiff, Swansea, Bristol	7·41
South-central	Shrewsbury, Birmingham, Hereford, Oxford, Reading	8.65
South-west England	Portsmouth, Salisbury, Plymouth	9.21

Vernon and Straker suggested that defective colour vision may be racially connected with pigmentation. Burt (1946), using a group of 649 children and one of 3,231 men, has shown that there is a small but well-established correlation between defects of colour vision and dark pigmentation of the iris. Pickford (1951) and Geddes, however, have reported investigations in which there is no evidence of a relationship between defective colour vision and skin and hair pigmentation.

TYPES OF DEFECTIVE COLOUR VISION

'Probably no single classification (of colour vision defects) will satisfy all proponents of the various theories of colour vision', says Chapanis (1949). The terms used in the common classification of the more easily separable types of defect 'were established on the assumption of the truth of the Young-Helmholtz or three-colour theory' (Pickford, 1951). This classification is based essentially upon the number of primary colours, used separately or combined, which are required by the individual to match all the colours of the spectrum. There are three main groups: (1) the *anomalous trichromats* who, though resembling normal people in requiring at least three primary colours to match all parts of the spectrum, require them in abnormal amounts (they are sometimes called the *colour-weak,* but a better designation is the *colour-different*); (2) the *dichromats*, requiring two colours (the *colour-blind*); and (3) the *monochromats*, requiring one colour (the *totally colour-blind*).

(1) *Anomalous trichromats*

are people who confuse large sections of the spectrum. They show wide variations from the normal in the proportions of the primaries they require in order to achieve satisfactory matches. The commonest are those who require abnormal amounts of red and green light to match a given yellow light. These may be divided into two sub-groups: (a) *deuteranomals* (or *partial deuteranopes*); and (b) *protanomals* (or *partial protanopes*). Deuteranomals require much more green than the normal in mixing red and green to match a given yellow. They form the more numerous group. Waaler and von Planta found that four or five out of every hundred males in their samples were deuteranomals. Among females the incidence was between three and four in every thousand. Protanomals require much more red than the normal in matching a given yellow. According to Waaler and von Planta, one in every 100 to 150 males is a protanomal. A third sub-group of anomalous trichromats is sometimes mentioned: (c) *tritanomals.* The tritanomal is said to be a person who requires much more blue or violet than the normal to duplicate the spectral colours. This is a relatively rare group.

Chapanis (1949) observes that 'it is generally believed that anomalous trichromats occur as intermediates of various grades between dichromats and colour normals'. Willmer (1946), for example, says 'they seem to form a graded series from the completely normal to the defectives constituting the two main groups of dichromats'. Pickford (1951), however, states that 'there is a gap between normal subjects . . . and those who have major red-green defects'. Because of the 'discontinuous nature of colour vision variations' it is possible to devise relatively crude tests of colour vision which operate at the level of this gap. 'Unless this gap were a prominent feature of colour-vision variations, such crude tests could never have been of any practical service'.

(2) *Dichromats,*

also, confuse large sections of the spectrum. They can match any part of the spectrum with a mixture of two primaries, and can match certain hues with white. The majority of dichromats are often referred to collectively as the *red-green blind*. These can be divided into two sub-groups: (a) *deuteranopes,* sometimes called *green-blind*; and (b) *protanopes,* sometimes called *red-blind*. Both confuse green, yellow and red. The deuteranope, however, sees the whole of the normal spectrum, and matches blue-green and a purplish-red with white, while the protanope sees a spectrum darkened at the red end, and matches blue-green and a red-purple with white. According to Waaler and von Planta, one in every 70 to 100 males is a deuteranope, the protanopes being slightly less numerous. However, Pickford (1954a) used an anomaloscope to classify an approximately random sample of 142 men and women and found that 'although deutans (deuteranopes and deuteranomals taken together) are more than twice as frequent as protans (protanopes and protanomals taken together), there are three times as many protanopic as deuteranopic dichromats'.

Two further sub-groups, sometimes called collectively the *blue-green* or *yellow-blue blind,* have been distinguished: (c) *tritanopes,* sometimes called *violet- or blue-blind*; and (d) *tetartanopes*. Both are very small groups. Wright (1952) has estimated the incidence of tritanopia as, probably, one in 65,000, the condition being about half as common again among men as among women. At one time the condition was thought to be largely due to disease, but Wright shows that the defect is of a congenital nature. The tritanope confuses blue and green, does not see violet as distinct from blue, and matches yellow with white. To the tetartanope, the whole spectrum appears in terms of red or green. This condition is said by Willmer (1946) to be extremely rare.

(3) *Monochromats*

are totally colour-blind individuals who apparently see the whole spectrum in terms of light and shade without colour or, alternatively, who perceive all in shades of one colour. Willmer (1946) says 'their vision is presumably similar in character to that experienced by normal people in twilight o moonlight; it may, of course, be brighter and probably it is, but it is apparently no more coloured'. Total colour-blindness, sometimes known as *achromatopia*, is very rare. Linksz (1952) using data from Sweden, estimates that one person in 300,000 is totally colour-blind. Parsons says that males are affected about twice as often as females. In a very few cases (*photopic monochromats*) the condition is said to arise as the result of disease. Most of the totally colour-blind, however, are *scotopic monochromats* and their condition is regarded as congenital and inherited. The subjects, says Parsons, usually have very bad central vision (less than 6/18), marked photophobia, and nystagmus. Refractive errors are common, generally moderate myopia, and correction produces little improvement of vision. Sloan (1954), who examined sixteen complete monochromats, found photophobia present in every case, and a range of visual acuity from 20/70 (one case) to 20/200 (nine cases). Gross nystagmus was observed in only four subjects, however, all under sixteen years of age, and there is a suggestion that this symptom decreases over a period of years.

THE GENERALLY CONGENITAL NATURE OF DEFECTIVE COLOUR VISION

Defects of colour vision are, with rare exceptions, inherited. 'All forms of red-green blindness are products of discontinuous variation and are subject to sex-linked inheritance' (Pickford, 1951). They are generally transmitted through the female, who is herself not usually affected. An unaffected male never carries colour-blindness. The defect of an affected male will be transmitted through his daughters, but they will not themselves be affected unless his wife is a carrier, when half will be affected, or unless she is herself colour blind, when all will be affected. His sons will not be affected, whether or not he is colour-blind, unless his wife is either a carrier or colour-blind. In such family histories as have been investigated, the type of defect transmitted is always the same, subject to certain laws of segregation which apply to this type of Mendelian recessive (Pickford, 1951, 1954a). 'Yellow-blue defects, however, are subject to continuous variation and are not sex-linked' (Pickford, 1951). Kalmus (1955) has, in fact, shown that tritanopia is a dominant characteristic.

There are occasional cases of defective colour vision that are the result of disease or injury to the eye or brain, but, says Farnsworth (1950), such disturbances are accompanied by more serious visual defects. Colour vision is temporarily destroyed during an attack of jaundice or tobacco poisoning, following the injection of santonin, or the instillation of adrenaline or atropine. Testing of colour vision should, therefore, be deferred when any of these conditions is present.

THE OCCUPATIONAL IMPLICATIONS OF DEFECTIVE COLOUR VISION

Danger to industry lies in the fact that a surprising number of men reach maturity without discovering that they have defective colour vision. Many do not realise the existence, or at least the extent, of their defect until they have made a serious mistake and perhaps caused an accident. Defects of colour vision cannot be cured, nor can they be corrected by the use of, say, coloured filters. However, colour defective men can safely be entrusted with many specialised jobs if they appreciate their defect and learn that they must never make a colour judgment.

For relatively few occupations are standards of colour vision set. The Metropolitan Police, though few other police forces, insist on normal colour vision, as do British Railways for all except clerical staff and others not concerned with moving traffic. Preventive Officers in the Customs and Excise Department, and staff in the Electrical Engineering Branch of the Post Office, are required to pass colour vision tests. In the Merchant Navy, the Royal Navy, and the Royal Air Force standards of colour vision are set for a number of jobs. But many employers, report the Colour Group of the Physical Society (1946), are either unaware of the problems that can be created by colour defectives or are content to tackle these problems in an *ad hoc* fashion, by transferring to other jobs workers found to be affected.

In the vocational guidance or placement of the colour defective it is important to know the nature and extent of the defect. No colour defective, for example, could be satisfactorily employed on colour inspection or grading tasks in textile dyeing and printing, or in the manufacture of plastics, paper, printing inks, paints, dyestuffs and similar products. (In all these industries there will, of course, be other tasks that can be performed by the colour defective.) The *anomalous trichromat*, however, may well, with good illumination, be able to see the difference between colour-coded wires used in the manufacture of telephone equipment, motor vehicles, or radio and electronic gear. He would probably, though, be unable to distinguish them under poor illumination and when the wires had become soiled by use, and so would be unsuitable for repair or servicing work on the same equipment.

Again, it is possible that the anomalous trichromat who would not be accepted for certain work in the Armed Forces could be considered safe for similar work in civilian life. Pickford (1954b), however, has pointed out that, 'on account of the possibility of their going through life without knowing of their defects, PA (protanomalous) and DA (deuteranomalous) subjects, far from being 'safe defectives', are particularly dangerous, because they tend to have confidence in their own colour judgments, and even resist any suggestion that they are not normal'.

The *dichromat* is clearly handicapped in the performance of many tasks. He could not, without mishap, work as a filing clerk if he had to operate a colour-key system of filing; could not safely distinguish colour-banding on pipes, or colour-coding on gas cylinders; could not wire coded electrical components without error. Pitt (1935) has shown that, out of 43 trichromatic colours extensively used in the paint trade, deuteranopes can distinguish only fifteen, and protanopes only twelve. If the number of colours had been increased to five hundred, the deuteranope could never have distinguished more than 28; the protanope never more than eighteen.

The dichromat would be undependable in the handling of clothing an furnishing materials, whether in retail or in wholesale distribution. He would be likely to make mistakes in copying or working from coloured plans, or in reading maps. He would be handicapped, particularly if a protanope, in photographic processing or process engraving. (The printing and photographic industries are among the few in which firms can be found using colour vision tests as a matter of routine in their selection procedures.) The protanope's peculiar difficulty is the time he takes to become adapted to red or yellow lights in the darkroom. Chapanis (1946, 1947) has shown that, whereas dark adaptation curves for deuteranopes and deuteranomals are essentially identical with those of persons with normal colour vision, the cone portions of those for protanopes and protanomals measured with red, reddish-orange, yellow, and white lights are higher than those for the colour normal. The discrepancy is most marked when measured with red light, and decreases as the wavelength of the light decreases. There is no evidence to support the claim, sometimes made, that colour blind people can see better in the dark.

The dichromat would also be unable satisfactorily to observe chemical reactions or to make colour assessments of temperature. He would make mistakes in horticulture, in the identification of plant diseases, flower varieties and in judging the ripeness of fruit. In distinguishing traffic signals he has to use his knowledge of the relative positions of the red and green lights, which may prove hazardous in an emergency. The protanope has an additional clue, in that he sees the green signal as bright and the red one as dim, but he may find it very difficult to see a red traffic signal or a rear light during night driving. The dichromat will also be at a disadvantage with the coloured lights which are, to an increasing extent, being used as signals for action for workers, notably in the chemical processing and oil refining industries.

However, it may be possible to modify a job so that it can safely be performed by colour defectives. Riddell (1949) has observed that 'in American main-line railroads, there is an increasing tendency to replace red and green signal lights by yellow lights which are placed above the track. The three lights appear vertical for go and change to horizontal for stop. This overcomes many of the basic difficulties, and is a relatively simple arrangement involving five lights. When several lines are running parallel to each other, any danger of confusion is overcome by placing the lights above the lines'. Similarly, 'in certain States in America, the red and green traffic lights have thick ribbed glass, which elongates the circular light, making the red tend to be horizontal and the green vertical'. Wright, in the discussion on Riddell's paper, commenting on the practice of coding resistors, according to their value, by a coloured dot painted against a coloured background, has said that 'their identification was found to be easier when glossy paint was used rather than matt paint'. Again, in some situations the chances of colour confusion might be reduced by combining or replacing a colour code with a shape code, or supplementing a light signal by an audible one. In vocational guidance or selection, the generally low-grade vision of the *monochromat* is of more consequence than the fact that he is totally colour-blind.

It should be remembered that the colour vision requirements of an occupation may change as an individual progresses in it. Wright (again see Riddell, 1949) observes that there are some industries in which 'although, if he were defective, his poor colour vision might not debar him from undertaking some of the humbler jobs . . . , he might in course of time find that he was unfitted for the exalted positions for which his seniority ought to have entitled him'. In those industries, such as paper-making, textile dyeing, and printing, where very fine colour discrimination is required, minor variations of colour sensitivity may be very important. Individuals displaying those minor variations are not detected by the Ishihara test, and more elaborate procedures must be used, such as Pickford's Four-Colour Test (Pickford, 1949a).

THE ISHIHARA TEST

The commonest type of test for detecting defects of colour vision consists of *confusion charts,* also known as *polychromatic* (many coloured) or *pseudo-isochromatic* (apparently equally coloured) *plates*. In these confusion charts the colours are carefully chosen to fall within the areas where colour defectives are most likely to be differentiated from normals. The best known of the tests of this type are: (a) *Stilling's Pseudo-Isochromatic Plates for Testing the Colour Sense*; (b) Ishihara's *Tests for Colour Blindness* (developed from Stilling's work); and (c) the American Optical Company's *Pseudo-Isochromatic Plates for Testing Perception* (a selection of plates from those prepared by Stilling and Ishihara).

Of these the Ishihara test is the most widely used. It is designed only for the detection of red-and-green-blind, and red-and-green anomalous, individuals, and the totally colour-blind. In the tenth edition (1951) it consists of thirty-eight plates. The first twenty-five of these are intended for testing literates; the remaining thirteen (plates 26 to 38) are for use with illiterates. All the plates are made up of patterns of differently coloured dots, varying in diameter from one to five millimetres. In the first twenty-five plates one or two digits are embodied in the pattern; in plates 26 to 38, one or two winding lines.

Plates 1 *and* 38, consisting of orange dots on a grey background, are intended for demonstration and for the detection of persons attempting to simulate defects of colour vision. The remaining plates are in six series. In the section of the test designed for literates there are four plates in each of Series 1 to 6; in that designed for illiterates there are two plates in each of the series.

Series 1 (plates 2 to 5, and 36 and 37) can be called a *transformation* series. In these plates a digit or a winding line made up of blue-red and yellow-red dots is seen by a person with normal colour vision on a background of yellow-green and blue-green dots. The dots vary both in chroma and in brightness. It is claimed that, to the red-and green-defective, only the *blue* red and blue-green dots stand out, the yellow-red and yellow-green being indistinguishable. The pattern is thus transformed so that the red- or green-defective, instead of seeing a reddish pattern on a greenish background, sees a bluish pattern on a yellowish background. Thus, in plate 2, where the normal sees an eight, the defective sees a three. The totally colour-blind sees no pattern at all.

Series 2 (plates 6 to 9, and 34 and 35) is another transformation series, but with background and pattern colour reversed; that is, blue-green and yellow-green dots form the digit or the winding line on a yellow-red and blue-red background. As before, the red- or green-defective should see a bluish pattern on a yellowish background, and the totally colour-blind see no pattern.

Series 3 and 4 are of the *vanishing pattern* type. In *Series* 3 (plates 10 to 13, and 32 and 33) the pattern is formed of yellow-red dots on a background of yellow-green and blue. In *Series* 4 (plates 14 to 17, and 30 and 31) the pattern is of blue-green dots on a background of blue-red and yellow. Though the digits or winding lines are plain to those with normal colour vision, they are rarely seen by those with defective colour vision, unless the defect is slight.

Series 5 (plates 18 to 21, and 28 and 29) is a series of plates containing a *hidden pattern* which is said to be seen only by red- or green-defectives, and not by people with normal colour vision or by the totally colour-blind.

Series 6 (plates 22 to 25, and 26 and 27) contains a series of *differential patterns*. Each plate contains two digits or two lines, one in red and one in red-purple dots on a background of grey dots of different brightnesses. It is claimed that protanopes generally see only the red-purple (the two in plate 22). Protanomals and deuteranomals, and those with normal colour vision, should see both patterns.

THE LIMITATIONS OF THE ISHIHARA TEST

Because of their great convenience and apparent simplicity of administration, polychromatic plates have been widely used for many years both in the Armed Forces and in industry. These tests have a spurious ease of administration, and American studies have indicated that, in the way they are generally employed, polychromatic plates are only about 50 per cent. effective.

Though defects in the tests themselves—such as changes in the printing inks used—account for part of the discrepancies encountered, more important sources of error are:

(a) careless administration;
(b) faulty interpretation of the results; and
(c) expecting more from the tests than they are capable of yielding.

The main source of error, however, is:

(d) disregard of the critical importance of illumination.

Hardy (Hardy, Rand and Rittler, 1945a) has said that, 'as they are at present administered, polychromatic tests have become as much tests of the lighting system used as they are tests of the subject's colour discriminatory ability'. Some people think that colour vision tests should be given in the particular lighting in which the workers have to judge colours. This is a fallacy. The colours of polychromatic plates are prepared for use under 'average daylight'; that is, illumination with a colour temperature of approximately 6500 to 6750° K. This is light such as is reflected by an overcast sky with little or no sunlight. Direct sunlight, which has a colour temperature of about 5400 to 5800° K, and light from a clear blue sky, 12,000 to 26,000° K, are unsuitable for the administration of confusion tests. The generally available forms of artificial lighting must not be used. Hardy, Rand and Rittler (1946) have shown that, when administered under illumination from a tungsten filament lamp (colour temperature about 2750° K), the efficiency of the Ishihara test is seriously impaired. The efficiency of plate 6, for example, in detecting deuteranomals falls from 100 per cent. under normal daylight to 43 per cent. under tungsten filament light, while there is a tendency, in Series 6, for protanomals and protanopes to be wrongly classified as deuteranopes. So-called 'daylight' lamps, which have a colour temperature of about 3575° K, are little better.

HANDLING THE ISHIHARA TEST BOOK

Because the printing inks used in the production of the Ishihara plates are not permanent, it is very important that they should be exposed to light as little as possible. When not in use the test book should be kept in its protective cover, and in a cupboard or a drawer. It should never be left unprotected on a desk.

Similarly, the plates must be protected against dust and dirt. The pages of the test book should be turned over by their corners. The subject must not be allowed to handle the book, nor should he be allowed to finger the plates. When it is necessary, as in plates 26 to 38, for him to trace a line, he should be provided with a soft, clean and dry artist's brush, or a clean round-ended glass or Perspex rod. A pencil or a stick that cannot be kept clean must not be used.

The plates should not be covered with Cellophane or similar materials. Such materials may alter the spectral quality of the light reflected by the plates and, at best, they give rise to confusing reflections.

GIVING THE ISHIHARA TEST

The test book should be held so that the subject, whether sitting or standing, can see the plates squarely. It should be between two and three feet from the subject's eyes, and arranged so that light falls on it from above or behind the subject. The book should be maintained in a fixed position and not moved about, because of the differential reflection of the inks used for the plates. Pickford (1951) has observed that 'figures invisible from immediately in front may be clear from one side but not from the other. It is comparatively easy to gain help in doing the Ishihara Test . . . by tilting the plates until some of the figures can be read by the varying intensity of the light reflected at different angles. Many colour-blind subjects habitually use this dodge and must be checked from tilting the plates or moving the head from side to side'. A book-rest is helpful in keeping the test book steady, and also enables the tester to have one hand free for recording the subject's responses. These should be recorded systematically on a suitable answer sheet.

Until a tester is quite familiar with the test, he should record the subject's responses *verbatim*. Eventually, however, he may indicate normal responses by an 'N', though abnormal responses should always be recorded in detail. (It is preferable to use an 'N' to indicate normal responses rather than a tick, which may give the subject the impression that there are right and wrong answers.)

When the test is being given the subject should be told:

> *I want to see how good you are at picking out colours. I'm going to show you some drawings like this* (turning to plate 1) *and I want you to tell me what you can see.* (Elicit response 'Twelve'.) *That's the idea. Now, in the other drawings you may see two numbers, or only one, or you may see nothing at all except a jumble of dots. Whatever you see, just call it out straight away.* (Continue with the remaining plates.)

If the subject is known to be illiterate, or experiences difficulty with plate 1 because he cannot read numbers, he should be shown plate 38. When he has responded to this by saying that he can see a wiggly line, he should be handed a clean artist's brush or a glass rod and told:

> *Will you just trace it for me with this? Show me how it goes.*

When he has done this, say:

> *That's the idea. Now, in the other drawings you may see one or two wiggly lines, or you may see nothing at all, except a jumble of dots. Whenever you see a line, just show me how it goes. Don't touch the drawings with your fingers. Just use the brush* (or *pointer*). (Continue with the remaining plates.)

Once the nature of the task has been explained to the subject, he should be given no further help. Particularly, he should not be told when he makes an abnormal response or a mistake, such as misreading a 3 for an 8. Any hesitancy in responding should be recorded.

The order in which the plates are presented may be varied from time to time, either as a matter of routine or if it is thought that a subject has memorised answers given to him by earlier subjects. It is probably unwise, however, to alter the order in which the series are presented, though the sequence of plates within those series may be changed.

After the subject has attempted all his plates (either plates 1 to 25 or plates 26 to 38, according to his degree of literacy), he should be re-examined with those plates he has missed, or where he hesitated appreciably, or made an abnormal response or an error. Where it is thought necessary, a subject who has attempted plates 1 to 25 may also be given plates 26 to 38.

INTERPRETING ISHIHARA TEST RESULTS

The need for care in administering the Ishihara test and in evaluating a subject's performance has been demonstrated in studies by Chapanis (1948), Hardy, Rand and Rittler (1945 a and b), and Pickford (1949b, 1950). Many of the individual plates do not give the clear-cut differentiation between normals and colour defectives that is often expected by those inexperienced in the use of colour vision tests. For example, Chapanis has shown that, in series 5, many persons with normal colour vision can see the 'hidden pattern'. In a group of 86 normals, 69 (approximately 80 per cent.) could see the pattern in plate 20. Even in plate 21, where only 41 per cent. could see the pattern, the 'normal response' was made by only 15 per cent. The remainder gave replies suggesting that they could see one or two digits, but not very clearly. It has also been found that both normal and colour-blind persons misread some plates because of the poor legibility of the digits.

However, Hardy, Rand and Rittler (1947) have shown that it is possible, using the number of normal responses made, to separate clearly persons with normal colour vision and those with defective colour vision. Using 74 colour defectives and 32 'low normal' subjects (that is, people with low colour discrimination not amounting to a real colour defect), they found that no defective made more than *fourteen* normal responses to plates 2 to 25, and no normal subject made less than *eighteen* normal responses. This held good for the fifth, seventh and ninth editions of the Ishihara test. Data are not available for the tenth edition, but it would appear reasonably safe to use the following cut-off scores, at least until further data are available:

On plates 2 to 25, diagnose as colour defective those making *fewer than fifteen* normal responses.

On plates 26 to 37, diagnose as colour defective those making *fewer than eight* normal responses.

On plates 2 to 37, diagnose as colour defective those making *fewer than twenty-two* normal responses.

In borderline cases, the tester should ask the subject to compare plates 10 and 19, 13 and 20, 14 and 18, or 17 and 21, and to say which plates are easier to read. A person with normal colour vision should find plates 10, 13, 14 and 17 easier to read; a colour defective should find plates 19, 20, 18 and 21 easier.

The number of normal responses made cannot be used to differentiate *types* of colour vision defect, nor can it be used to assess the *extent* of a defect. Hardy, Rand and Rittler (1945a) found that, while in general dichromats gave less normal responses than anomalous trichromats, 65 per cent. of deuteranomals and 75 per cent. of protanomals gave between no and four normal responses, the range which included 100 per cent. of the deuteranopes and 94 per cent. of the protanopes tested.

The plates in series 6 are, however, designed to facilitate the classification of subjects according to type of colour vision defect. According to Ishihara (1951) the deuteranope will be able to read only the red digit, the protanope only the red-purple digit, while the anomalous trichromat and the normal will be able to read both digits. This claim is not supported by the work of Hardy, Rand and Rittler (1945a). Using plates 12 and 13 from the fifth edition (plates 22 and 23 in the tenth edition), they obtained the following results:

> Of 15 deuteranopes, all were diagnosed by one or other of the plates.
> Of 17 protanopes, 12 were diagnosed as protanopes, 1 was diagnosed as a deuteranope.
> Of 23 deuteranomals, 17 were diagnosed as deuteranopes.
> Of 12 protanomals, 10 were diagnosed as protanopes, 1 was diagnosed as a deuteranope.

It is clear that not only dichromats, as claimed by Ishihara, are detected by the plates in series 6, but also many anomalous trichromats (in this investigation, more than three-quarters). Moreover, though no deuteranope or deuteranomal was diagnosed as other than green-blind, one protanope and one protanomal were diagnosed wrongly as being green-blind. This tendency to mis-classification increases greatly with yellowish illumination. These findings have been confirmed by Hardy, Rand and Rittler (1945b) using plates in series 6 of the seventh edition and the British reprint of the ninth edition, by Pickford (1951) using the eighth edition, and by Chapanis (1949) using the Japanese ninth edition.

Several workers have suggested that a shortened form of the Ishihara test might be used to screen red-green defectives from those with normal colour vision. Pickford (1950) has proposed that plates 10, 11, 14, 15, 18 and 23, together with plate 1 as an introduction, might be used for this purpose. He found no overlap in the distributions for 239 normal subjects and those for 145 colour defectives. Three or more 'definite' errors (that is, abnormal responses exclusive of any misreadings) would be, says Pickford, sufficient to fail major defectives. This shortened form of the test is, though, no better than the full test for detecting particular types of defect. However, as Pickford (1951) points out, it would have 'the advantages of quickness, and of unequivocally separating major defective subjects from the normal and minor defective types, but it would have the serious disadvantage of being very short'. The answers might easily be learned by the subjects wishing to deceive the tester, and the remaining eighteen plates would have to be retained for use should any difficulty arise.

Properly administered, the Ishihara test is a good rough device for screening red-green defectives. It is one of the best of the pseudo-isochromatic tests. However, no attempt should be made to use it for the classification of subjects as to type or extent of colour defect. If information of this sort is required, use must be made of more efficient (and more time-consuming) methods of measuring colour vision weaknesses, such as the anomaloscope (Pickford, 1951).

COMMUNICATING TEST RESULTS TO THE COLOUR DEFECTIVE

Unless they have been tested, men with defective colour vision are often unaware of their condition. They see all the things that other people see

Book Reviews

The Study of Behaviour: Q-Technique and its Methodology. By WILLIAM STEPHENSON.
Chicago: University of Chicago Press, 1953. Pp. 10 + 376. Price $7.50.

The analysis of human behaviour presents a far more complicated problem than is generally realised. Every measurement which the psychologist obtains is by its very nature liable to vary in three respects. As Stern put it forty years ago, "a mental measurement is a measurement for (1) a particular quality or *trait* of (2) a particular *person* (3) obtained on a particular *occasion*. Hence in any statistical research the traits may differ; the persons may differ; and the occasions may differ. To keep the issues as simple as possible the psychologist usually holds one of the three variables constant, and then averages out the variations in one of the other two by means of a product-sum formula. In this way he is able to secure a set of correlations between variables of one type only—between different traits (T-technique), between different persons (P-technique), or between different times or occasions (O- technique). However, each kind of correlation can be calculated in two distinct ways. Thus, if we confine ourselves to measurements obtained on a single occasion, we may either average the persons and correlate the traits, or average the traits and correlate the persons. And similarly with the others. In all therefore there are six methods of obtaining correlations, and six ways of planning a factorial research.

Dr Stephenson's book is concerned chiefly with the first two procedures—those in which the data are obtained on a single occasion only. From time to time he refers to the others; but he does not, I think, make it clear that in doing so he is passing to another kind of technique, which involves all the difficulties of correlating time-series. The method most commonly employed is the correlation of traits, called R-technique by Dr Stephenson. But the others have been in occasional use from the begining of factorial research.

To overcome what he considers to be the defects of the older forms of 'P-technique', Dr Stephenson has developed certain devices of his own. These changes, he believes, provide us with an entirely new statistical procedure, which he has christened Q-technique. He rightly complains that the recent custom of using 'Q-technique' as a label for every kind of factorial research that starts by correlating persons is not only confusing to the reader but unfair to himself and to his predecessors. And the first part of his book consists in an endeavour to compare the relative merits of the different techniques, and more particularly to defend his own proposals against the objections which Professor Cattell and other factorists have raised. The second part is concerned with 'practical applications'. Its purpose is, not so much to survey results already achieved by using Q-technique, as to present a comprehensive programme of experiments with instructive illustrations of each type.

Throughout Dr Stephenson writes with a lively and amusing pen. His style is perhaps not always as lucid or as logical as the reader could wish; and at times the reader may detect signs of carelessness or haste. Thus, when examining my own earlier efforts at factorizing correlations between persons, he quotes several paragraphs which he describes as "a statement by Burt"; in point of fact, however, the statement was written, not by me, but by Mr Babington Smith, who was not referring to my views, but (as he says) "attempting to make Dr Stephenson's position clearer". There are, too, several slips in the arithmetic and algebra. But these are minor defects. Whether or not the industrial psychologist will be able to follow or accept the arguments set out in the theoretical chapters, he can certainly study with profit the original and ingenious suggestions put forward in the second half of the book. Much of the discussion has a direct and obvious bearing on problems of vocational guidance and selection; and the experimental schemes proposed well deserve trying out on a more extensive scale.

CYRIL BURT

Method and Theory in Experimental Psychology. By CHARLES E. OSGOOD. London: Geoffrey Cumberledge, The Oxford University Press, 1953. Pp. 800. Price £4.

Most of us pay lip service to the view that psychology is in its infancy, an immature science offering a very incomplete knowledge of ourselves. Yet this is no understatement, but a defamation of the truth. The information already available about ourselves is voluminous, and the present book confirms one's suspicion that it is beyond the grasp and comprehension of any single author.

The book will inevitably be compared with its dated though famous forerunner, Woodworth's *Experimental Psychology* (1938) as well as to the recently published *Handbook of Experimental Psychology* (1951) edited by S. S. Stevens. The former, which had attained the enviable status of a widely prescribed textbook for over a decade, deliberately aimed to instruct in the techniques and findings of the laboratory from its earliest beginnings, but failed to show the way experimental results were intimately related to general theory. On the other hand, the latter, with its impressive array of specialists in many fields, and including chapters on engineering psychology, selection and training procedures, and work and motor performance, suffers from a lack of continuity.

It is these deficiencies that Osgood has attempted to remedy. The coverage is, on the author's own admission, selective. He writes, "I have tried to maintain throughout a certain continuity of approach, stressing those studies most pertinent to theoretical issues". If, therefore, bias is detected in the handling of theoretical issues and empirical data, warning has been given. Osgood studiously avoids the applied fields and writes primarily for the intelligent student, anxious in his final year of undergraduate study to obtain oversight and 'approach'.

Formally the book falls into four parts: sensory processes, perceptual processes, learning and symbolic processes. In actual fact it would seem to be dominated by two themes: the first, that some perceptual phenomena can be explained in terms of well substantiated peripheral physiological mechanisms. Thus, figural after-effects, made famous through the studies of Gibson and Koehler, can in part be accounted for by an extension of Marshall and Talbot's analysis of the neural mechanisms in vision, especially physiological nystagmus, *i.e.*, the finding that the eyes are in constant fluttering motion, even during visual fixation, so that neural excitation oscillates round a mean position. The second theme is that, contrary to the ultra behaviourist theories, many perceptual phenomena, as well as learning, require centralist concepts such as those favoured by some of the Gestalt psychologists and Hebb.

Osgood's solution to the bickerings of Gestalt versus Behaviourism is his own theory —the mediation hypothesis. The mediation hypothesis takes into account the obvious fact of cortical intervention between stimulation and overt response. Stimuli normally giving rise to one set of responses, can be routed into other channels through conditioning. They become signs when they are conditioned to reactions which otherwise are made on the basis of interaction with a stimulus-object. The important point—which distinguishes Osgood's position from ordinary Pavlovian conditioning theory—is that signs evoke only part of the responses evoked by the objects of the sign. Furthermore, sign-responses are a source of additional stimulation leading to more sign-responses, etc. By following up this view, a theory of meaning and symbolism emerges which enables the author to propose a reconciliation of Hull's and Tolman's learning theories. Osgood has much in common with Hebb and other writers who have recently tried to reintroduce neurophysiological explanations into psychology. As it stands, his view leaves many gaps, but promises to reinforce current thought on the subject.

The size of the book enforced its completion over a number of years and prevented the inclusion of much recent material. Thus, from internal evidence, it seems that the section on learning, in which the mediation hypothesis is first described, was written between 1949 and 1950. The section has not been seriously revised and is therefore dated in relation to some of the books already available on this topic. The lack of any reference to the work of Harlow on learning in monkeys and a related discussion of the concept of 'set' represents a serious omission. Equally serious is the scant reference to Skinner's work on the effect of patterns of reinforcement on behaviour, and the summary treatment of the problems raised by the finding that the spacing of rewards for work leads to greater persistence of effort. His orientation is such that there is no discussion of feelings and emotions, conditioned neuroses or Freudian and other theories of abnormal behaviour. In the section on sensory processes, which occupies 200 pages, Helson's adaptation level theory finds no place. Perhaps the gravest fault of the 300 pages devoted to learning is the uncritical attitude to Hull's theory and the extension of this bonhommie to experimenters producing the right sort of experimental results.

The book makes for relatively easy reading, is well laid out and adequately illustrated with graphs and drawings. The student will be especially grateful to Dr Osgood for the graceful summaries of such examination topics as 'the nature of secondary motivation and reinforcement', 'figural after-effects', 'the time error', 'language development', 'thinking', etc. Occupational psychologists may find relaxation in pursuing with Dr Osgood some of the more rarefied themes of psychology. He has all but succeeded in giving a unified approach to heterogeneous material.

H. M. B. HURWITZ

The Social Psychology of Industry. By J. A. C. BROWN. London: Pelican Books, 1954. Pp. 310. Price 2*s*. 6*d*.

It has been said that all psychology is social psychology. This is either untrue or trivial, depending on what you mean by 'social'. In the absence of agreed definitions of the limits of social psychology—either by reference to subject matter, or to standards of scientific methodology—it is difficult to decide who is a qualified 'social psychologist' and who is just a 'psychologist discussing social issues'. Dr Brown is by training a physician and a psychiatrist. He was involved with War Office Selection Boards during the War, and for some years since then he has worked with a large industrial concern. (From occasional references in his latest book one gathers that this was Unilever.) He is a man of massive erudition, as is evidenced by the number and diversity of authorities he quotes. He is also a man with some very decided personal views. Some at least of these views are unlikely to be held with anything like Dr Brown's conviction by other psychologists.

His personal viewpoint seems to involve the following postulates: (1) The most important fact about human beings is that they are *social* animals. (2) Neurotic disorders are largely induced by the failure of modern society to provide a healthy psychological environment. (3) Industrial psychologists were a pretty dim and insensitive lot until Elton Mayo came along. (4) Most of them still are. (5) Managers are, in general, hypnotised by the idea of 'the economic man', and the economic man is a myth "in which every detail is almost completely fallacious" (page 16).

Such a frame of reference is not shared to any marked extent by this reviewer, who found himself increasingly antagonised as he worked, conscientiously but unhappily, through his second reading of the volume. So marked did this antagonism become that he was on the verge of condemning Dr Brown's literary style as both tedious and difficult, when objectivity happily reasserted itself and he tested this impression against Dr Flesch's 'Reading Ease' formula. The result was a clear victory for Dr Brown; the book is well within the readability range of the population of factory managers, time and motion study engineers, and interested laymen to whom its author addresses it. Dr Flesch has, unfortunately, not (yet at any rate) provided a complete guide to objective book reviewing, so the criticisms which follow may well be as baseless as the suggestion that the book makes difficult reading.

Dr Brown starts with a historical retrospect. Any historian is, at best, as good as his sources: at worst he is as bad as his prejudices, his preconceptions, and his deliberate selectivity. Dr Brown makes use of some excellent sources. But would many psychologists be prepared to agree that "Industrial psychologists have, in the past, taken too much for granted. The whole structure of industry, its traditions and superstitions, have been accepted almost without question, and one had the uncomfortable impression that human beings were being made to adapt to industry rather than the reverse" (page 20)? Or again, would many be prepared to take the austere viewpoint that "the industrial psychologist should, like the industrial physician, be on nobody's side" (page 20)? Those of us who try to earn our living as, perhaps rather hack, industrial psychologists do like to feel we are doing somebody some good. Chapter 2 is entitled 'Human Nature and Society'. It is in the Ruth Benedict–Margaret Mead 'Culture Pattern' tradition, but it is coloured with a Spenglerian gloom. Chapter 3 is on 'The Work of Elton Mayo'. Characteristically it starts, "The inadequacy of the assumptions on which most of the early work of industrial psychology had been based was first shown by the failure of certain experiments carried out at the Hawthorne Works of the General Electric Company in Chicago between 1924 and 1927". This seems, to put it mildly, to be taking the Relay Assembly experiment too seriously. Readers may care to be reminded that H. M. Vernon, writing on 'The Influence of Hours of Work and of Ventilation on Output in Tinplate Manufacture' (Industrial Fatigue Research Board Report, Number 1. HMSO, 1919) demonstrated that for five factories 20 per cent. fluctuations in output were very closely related to external temperature; and the better the ventilation where the men worked in front of the furnaces, the less were the fluctuations in output. This demonstration of the importance of physiological factors in relation to output in hot and heavy work is in no way upset by the failure to obtain similar results for six girls engaged on a repetitive light assembly job.

Chapters 4 and 5 are titled respectively 'The Formal' and 'The Informal Organisation of Industry'. Dr Brown seems to dislike formal organsation. He does considerably less than justice to the achievements of formal organisers, and indeed, apart from a brief discussion of the 'staff and line' principle, he says very little about formal organisation. Curiously he omits to discuss the subjects that have now acquired the title 'Industrial Relations'—*i.e.*, Trade Unions, Employers' Organisations, Consumer Councils, Government Regulations, Arbitration of Industrial Disputes, Company Law and so forth. He chooses instead to attempt a 'man from Mars' description of the social organisation of a 'typical' light fabrication and assembly factory employing some 1,500 to 2,000 people, and in doing so he draws heavily on the anthropologists' concepts of 'status' and 'role'.

Chapter 6 is a useful discussion of the nature and value of attitude and opinion surveys in industry. Chapter 7 on 'Work, its Nature, Conditions and Motivation', is a sensible and persuasive discussion of job satisfactions and incentives. In Chapter 8 Dr Brown is off on his charger again. His subject is 'Leaders and Leadership'. Many of his criticisms of the early psychological and sociological work on leadership are well merited, but they are all pretty negative. When he expounds his own views they seem to this reviewer to be but a small advance on the work he has condemned. He tends to a 'gestalt' view of leadership—*i.e.*, the leader's behaviour is more a function of the group in which he rises to leader role than a matter of individual character and ability. He is apparently wedded to the Lewinian classification of autocratic, democratic and laissez-faire. It is high time this arbitrary and misleading classificatory system was dropped. Studies of actual, as contrasted with laboratory, leader behaviour have failed to support this classification. In particular, the factorial studies by Hemphill at Ohio, and the work of Katz and his colleagues at Michigan, as well as that by Comrey, Pfiffner and associates at Southern California, have indicated three and possibly four independent dimensions of leader behaviour. Instead of a single continuum from autocratic to democratic, with laissez-faire as rag-bag into which to stuff observations which won't fit anywhere else, these more recent studies delineate the following independent dimensions: (1) consideration, *i.e.*, the extent to which the leader shows a concern for the welfare and satisfaction of his subordinates as individuals; (2) interaction facilitation, *i.e.*, the extent to which the leader organises and encourages his subordinates to act together as a harmonious team; (3) production emphasis, *i.e.*, the extent to which the leader stresses the importance of achieving definite targets—either in a helpful or in a 'needling' way; and finally, and somewhat less definitely, (4) membership character, *i.e.*, the extent to which the leader maintains social distance from his subordinates as contrasted with his willingness to be regarded as 'one of the boys'. This classificatory system promises to provide much clearer insight into the nature of leadership, and it has the notable merits of being both empirical and very largely free of value judgments.

At the end of Chapter 8, Dr Brown turns to methods of leader selection. His enthusiastic endorsement of Dr Henry Harris's book *The Group Approach to Leadership Testing* does little to commend him to this reviewer. Chapter 9 is on 'Frustration'. There is a lot of sensible material here, but so graphic and emphatic are the descriptions that the reader is likely to emerge—like the character in 'Three Men in a Boat' after reading a medical dictionary—with the feeling that a complete breakdown can be held off only for a few hours. Chapter 10 presents a summary and conclusions.

This is not a bad book, but it is not a balanced book. It is much more the expression of a point of view than a dispassionate analysis of 'the social psychology of industry'.

JOHN D. HANDYSIDE

Symposium on Human Factors in Equipment Design. Volume 2 of the Proceedings of the Ergonomics Research Society. Edited by W. F. FLOYD and A. T. WELFORD. London: H. K. Lewis, 1954. Pp. 132. Price 21*s.*

A conference of the Ergonomics Research Society cannot be an easy thing to condense into print. However pervading may be the unifying theme, in this case equipment design, the papers are by scientists from a wide variety of disciplines, some of whom are interested in theoretical implications, others in suggesting practical uses for their work. It was only after editing the proceedings of the second conference, on fatigue, that Dr Floyd and Mr Welford turned their attention to the first conference. If the results seem a little disjointed, one must bear in mind the editors' difficulties and the time-lag.

Among the more outstanding papers one must refer to that by Bengt Akerblom on 'Chairs and Sitting'. After criticising traditional chair designs, which seem to have changed little in 3,000 years, Akerblom makes clear, positive, quantitative recommendations as to the desirable size of a seat. All we need to do now is to make use of his suggestions. C. B. Gibbs's paper on 'Movement and Force in Sensori-Motor Skill' makes an important practical point about control levers. He compared two types: those in which the response of a mechanism depended on the distance through which a lever was moved, and those in which the response depended on the pressure applied to an almost stationary lever. The latter showed distinct advantages. Although this conclusion has an obvious importance to engineers designing machines, the paper is so written that few engineers would grasp the real point. Possibly physiologists or psychologists interested in the theoretical implications would have a more patient approach.

Several papers have an anatomical basis: 'Body Size and Work Spaces' (G. M. Morant) and 'Range and Strength of Joint Movement' (H. D. Darcus). Both these to some extent bear out Professor Le Gros Clark's comment that what we know of the human body is largely based on cadavers; academic knowledge offers relatively little information on live bodies. Darcus describes some of the new techniques now being used, and Morant deals with the operational approach of 'experimental trials' in planning limited work spaces such as aeroplane cockpits.

In his opening address Sir Ben Lockspeiser remarked, 'The reduction of the load placed on the motor system (that is, the reduction of muscular effort) has very often involved an increase in the load imposed on the sensory side'. One feels that the papers by R. Conrad, M. D. Vernon, and Norah E. Graham, all dealing with observation of displays or indicators, are of more importance than that by Per-Olof Astrand on 'Physical Working Capacity in Relation to Sex and Age'. The latter comments, 'The problem . . . is to determine the maximum rate of work that a man can support, day in and day out, and still retain vigour to an advanced age'. Some jobs still are hard and heavy, but the tendency is to get mechanical power on to them— 'Call up the little horses', to quote one advertisement. Research is surely better directed at problems of controlling the horsepower than at those of doing horsework by manpower?

'The Physiological Effects of Climate on Man' by O. G. Edholm forms a convenient summary for the non-physiologist (and possibly for the physiologist too?) of the heat regulation of the body, the effects of climate and the measurement of the heat insulation of clothing. The remark that 'a pair of boots weighing 5 pounds is equivalent to 20 pounds carried on the back' may be of passing interest to climbers. The paper by M. Nielsen and L. Pedersen describes experiments on heat regulation; the ingenuity of the method rather than the results may commend it to the reader not directly involved in this field. Any body, living or not, can lose or gain heat by radiation, convection and moisture balance; but since control and measurement on a living body can be difficult, in this case a 'clothed thermostat' of the shape and size of a person was used.

To anyone interested in problems of equipment design and the effect of environmental conditions, this book can be recommended. Some of the papers are immediately relevant, and of the others it can at least be said that they give a lead to those in search of information.

DAVID COX

Public Personnel Management. By WILLIAM G. TORPEY. New York and London: Van Nostrand and Macmillan, 1953. Pp. xii + 431. Price 37*s.* 6*d.*

The author of this book is Personnel Officer in the Naval Research Laboratory in Washington and a part-time faculty member of the School of Government in the George Washington University. What he writes is no doubt a version of his class lectures, and if so, his students got good value for their money. Some of them may have found it rather dull going, but on the other hand the clarity and system with which the very detailed material is presented are admirable; and the somewhat insipid style may have enabled the author to get away with a series of critical comments which would not have been allowed to someone who wrote in a more vivid and personal way.

The major portion of the book is taken up with the history, legislative background, administrative framework and rationale of major items of public service personnel practice. In each case the federal, state and local government levels are dealt with in turn. On the whole, *Administration* rather than *Management* would have seemed to the reviewer appropriate in the title, but all the chapters except the first conclude with a section headed 'Problems of Administration' in which managerial problems are discussed. The book is not, of course, addressed to the professional psychologist, but the author is a convinced 'human relations' man and his writing is informed by what most psychologists would consider the proper spirit and relevant knowledge.

On the view presented by Mr Torpey, American practice and problems are more like our own than might be supposed. An important difference, however, is the place taken in the United States system by the Civil Service Commission, which appears to act as a central staff agency, setting standards, giving a lead and exercising a loose general supervision on all aspects of personnel work. Another difference is the much higher rate of labour turnover in U.S. federal offices, 36 per cent. in 1951, about half of it being apparently due to superior opportunities in industry. A third interesting difference is in the use of open reporting which the author commends on the ground that "carefully and honestly appraising an employee's strong and weak points tends to strengthen and improve relations between the employee and the supervisor".

The reviewer noted several unexpected omissions. One of these was any specific discussion of honesty in civil servants who nowadays may have to handle business potentially worth a very great deal of money to interested parties and who can be subjected to quite subtle temptation. Another missing topic was the question of overseas service and compulsory transfers for public servants, which one might have thought would be as great a problem in the new imperial power as it is in the old. Perhaps less unexpectedly, women's questions, which are important here, do not seem to arise in Washington. But, finally, it was a real disappointment to find that the author did not deal with the employment problems of scientists in the public service; the problems are so real, and the author at his desk in the U.S. Naval Research Laboratory so well placed to appraise them.

N. A. B. WILSON

Evaluating Research and Development. Edited by IRVING R. WESCHLER and PAULA BROWN. Los Angeles: Institute of Industrial Relations, University of California, 1953. Price $1.65.

This book is an account of the proceedings of a one-day conference of 37 adminisorganised, and pitched at widely differing levels of psychological—and managerial—sophistication.

Evaluation should, however, be in the light of what the author attempted, and not of what the reader sought. Mr Seymour's aim "has been to provide for the manager or supervisor in industry an outline of the mental and bodily processes involved in the acquisition of skills and a guide book on how to set about training workers on repetitive operations". The reviewer, a psychologist with special interest in the practical problems of operator training, while personally disappointed at certain features, nevertheless welcomes this attempt to encourage a more observant, informed and inquiring approach to it.

It could be said there is something here for everyone interested in any degree in the subject. Illustrative examples are liberally described. The sections on supervision of training and on the training of instructors are unexceptionable. Those which discuss the nature of skill, the parts played for example by mental organisation, and by muscular and tactile sensitivity, will start fresh trains of thought for many. Discussion of training devices develops a new field, and outline timetables and record forms, and the description of cash savings which have resulted from the introduction of systematic training all have their place. But one cannot escape the feeling that the book falls between two stools. From his wealth of knowledge and lively stream of conjectures the author must have found it difficult to select only those facts and comments which space and his avowed objective warrant. To do justice to theoretical discussions more space and more documentation would be needed. To provide a busy man's guide to industrial training for manual operations would require a less discursive, more rigorous, selection and arrangement of material.

Mr Seymour does not overlook the fact that training needs are of many kinds; different, for example, for a large organisation with a fairly regular inflow of learners, for a small organisation whose intake is erratic, casual or seasonal, for emergency periods of increasing staff or retraining for new work, and so forth. They vary also with the complexity, the length, the delicacy, the importance of the operation. But he does not appear to discriminate adequately between techniques which might suit different circumstances. Rather than review and evaluate various methods, he tends to advocate the "progressive part" method which he had done so much to develop. This is of course legitimate, though one would prefer the intention to be explicit. But enthusiasm for successes to which this method doubtless contributed has perhaps caused him to overlook the fact that other accepted psychological hypotheses were always incorporated in the training programmes; and the effect of applying these principles or well accredited hypotheses concerning, for example, competition, target setting and the avoidance of fatigue and monotony, is not discussed. The more elaborate techniques and arrangements described undoubtedly have their place; but enthusiasts are warned to lay hold on common sense and perspective before imitating them. Over-development can lead to untimely death. Techniques which seem reasonable, which can be supported by conviction based on insight and personal observation, can probably be introduced with benefit; but at the present stage of development of knowledge of this field, it would be rash to adopt, on the strength of someone else's experience, methods which go against the grain, seem contrary to reason, and are doubtless difficult to "sell".

From the psychologist's viewpoint the book is unsatisfactory, though by no means negligible. (It must be remembered it was not written for psychologists.) It is neither a series of case studies, nor review of various training methods, nor an avowed exposition of one method nor a consideration of psychological findings about learning which might closely affect training. A list of references follows some chapters, but in others, notably chapter II, which purports to discuss the fundamentals of skill and the acquisition of skill, there are disputable statements for which no supporting evidence is offered. The psychologist will wish to join issue on many points, and the fact that response is provoked at all in a reader for whom the book is not intended may perhaps be counted a merit; he may wish that two volumes had appeared in parallel, the one theoretical, the other strictly practical. The foreword, by Professor T. U. Matthew of the University of Birmingham, disarms criticism by pointing out that compression, applied in the interests of shop floor serviceability, "may tend to make it appear that the author is very biased in regard to controversial problems in the subject matter". The "shop floor" reader should keep in mind that the faults of the volume result from the range of the author's experience and his generous desire to give much in little space.

Everyone interested in operator training should read this book; though the going may sometimes be dull, most readers may expect to find at least some gleams of light to compensate them for their trouble. I. J. BLAIN

Other Books Received

The following books have been received. Their inclusion in this list does not preclude their review later.

Some Young People. Compiled by PEARL JEPHCOTT. London: Allen & Unwin, 1954. Pp. 168. Price 12*s*. 6*d*.

Le Personnel de Cadre Subalterne. By OFFICE BELGE POUR L'ACCROISSEMENT DE LA PRODUCTIVITE. Bruxelles: Office Belge pour l'Accroissement de la Productivité, 1954. Pp. 72. Price 50 *frs*.

Training for Human Relations. By F. J. ROETHLISBERGER and others. Boston: Harvard University Graduate School of Business Administration, 1954. Pp. xviii + 198. Price $2.00.

Psychotechnique de la vente. By M. DE SCHAMPHELEIRE. Anvers: 93, Kolonielaan, 1954. Pp. 20. Price not stated.

The Comic Side of Being Paralysed. By MARGRIT WYDLER. Ilfracombe: Arthur H. Stockwell, 1954. Pp. 40. Price 2*s*. 6*d*.

The Sociology of Work. By THEODORE CAPLOW. Minneapolis: University of Minnesota Press (London: Geoffrey Cumberledge, Oxford University Press), 1954. Pp. x + 330. Price $5.00 (40*s*.).

Labor-Management Relations in Illini City. By W. ELLISON CHALMERS and others. Volume 1—The Case Studies. Volume 2—Explorations in Comparative Analysis. Illinois: Institute of Labor and Industrial Relations, 1954. Pp. xlii + 810; xii + 662. Total price $15.00.

Factorial Analysis for Non-Mathematicians. By C. J. ADCOCK. Melbourne: University Press; London: Cambridge University Press, 1954. Pp. 88. Price 17*s*. 6*d*.

Bases for Industrial Relations. By WALDO E. FISHER. Pasadena: California Institute of Technology, 1954. Pp. iv + 28. Price $1.50.

The Reach of the Mind. By J. B. RHINE. London: Penguin Books, 1954 (First Published 1948). Pp. 198. Price 2*s*. 6*d*.

Principles of Industrial Psychology. By THOMAS ARTHUR RYAN and PATRICIA CAIN SMITH. New York: Ronald Press, 1954. Pp. xiv + 534. Price $5.50.

Minds and Machines. By W. SLUCKIN. London: Penguin Books, 1954. Pp. 224. Price 2*s*.

Die Tests in der Klinishen Psychologie. Volume I. By ERICH STERN. Zurich: Rascher, 1954. Pp. viii + 418. Price Swiss *Fr*. 28.10.

Swanson Products Ltd.: Wards Foundry Ltd.: Fidelis Electronics Ltd. Case Studies by the CASE STUDY WRITERS CIRCLE. London: Newman Neame, 1954. Pp. 9; 5; 12. Price 21*s*. for 15 copies of one Case Study.

Handbook of Employing Interviewing. 3rd edition. By JOHN MUNRO FRASER. London: Macdonald & Evans, 1954. Pp. x + 254. Price 11*s*. 6*d*.

The Attack on Big Business. By J. D. GLOVER. Boston: Harvard University Graduate School of Business Administration, 1954. Pp. xvi + 376. Price 48*s*.

Fairer Shares. By JOHN SPEDAN LEWIS. London: Staples Press, 1954. Pp. x + 244. Price 8*s*. 6*d*.

The Load on Top Management—Can it be Reduced? By L. URWICK. London: Urwick, Orr & Partners, 1954. Pp. 52. Price 2*s*.

Obituary

BENJAMIN SEEBOHM ROWNTREE, C.H., LL.D.

Benjamin Seebohm Rowntree, C.H., LL.D., a Vice-President of the National Institute of Industrial Psychology, who was its Chairman from 1940 to 1946, died on 7th November, 1954, at the age of 83. Mr Rowntree had been associated with the Institute from its foundation and was, in fact, one of the small group of industrialists whose interest and practical financial support made the foundation of the Institute possible.

Mr Rowntree began to take an interest in sociological questions during his twenties, and his book *Poverty, a Study of Town Life* was published in 1901. This was the outcome of a remarkable survey of conditions in the city of York, in the course of which his investigators collected information from every working-class home in the city. Throughout his life he continued his sociological studies, which were reported in a number of books, the last of which appeared in 1951.

During the 1914-18 war, Mr Rowntree was invited by Lloyd George to become Director of the Welfare Department of the Ministry of Munitions. One of the consequences was the foundation, with his support, of the Industrial Welfare Society. It was he who began, in 1919, the meetings of managers and supervisors which came to be known as the Oxford Conferences. The Management Research Groups also owed their origin in 1926 to him.

His sociological interests, and his work on a number of special committees and for many bodies to which he lent his support, did not prevent him from playing a very full part in the management of the family business of Rowntree and Company Limited, of which he became Chairman in 1925. This was the proving ground for his ideas—and for his ideals—on policy and practice affecting relations between management and men. For example, he testified to his belief in the contribution that psychology could make to the human problems of industry, not only by supporting the Institute from its very inception, but by establishing in the early 1920s a psychological department in the factory at York. This was the pioneer example of the establishment of such a unit in British industry, and it is unhappily true that for many years the psychological department at the Cocoa Works remained unique; and, even now, there are only a handful of companies which have followed Mr Rowntree's example.

Mr Rowntree was a member of the Institute's Executive Committee, whose meetings he regularly attended from 1921 until 1948. In addition, he was always accessible to those of the Institute's staff who appealed to him for advice and practical help. In 1939, at the end of a period of acute financial difficulty, when the Institute's position had been retrieved through the generosity of a small number of faithful supporters, Mr Rowntree obtained for it, from an anonymous source, a donation of £5,000. It was this donation which enabled the Institute to accomplish the transition to war conditions and subsequently to embark on post-war developments which laid the foundation for its future expansion. This is the measure of its debt to him.

Sincerity and kindliness were perhaps the first impressions one received on meeting Mr Rowntree. Then followed the realisation of his remarkable width of interest and knowledge, of his tact and humour. He was a humanitarian, but not a sentimentalist. He put people before things, but he never lost sight of practical matters. At my last meeting with him at his home at High Wycombe he remarked that his advice to his fellow industrialists had always been that, even if they put economic considerations first, it was good business to treat their workers as human beings because human beings responded to fair and just treatment by becoming more efficient workers. For him it was justice and fairness which came first.

C. B. FRISBY

OCCUPATIONAL PSYCHOLOGY

APRIL 1955 VOLUME 29: NUMBER 2

A Method of Interviewing used in Studies of Workers' Attitudes:

II. Validity of the Method and Discussion of the Results

By R. MARRIOTT and R. A. DENERLEY

Mr Marriott is an assistant director and Mr Denerley was, until recently, a member of the Medical Research Council's Industrial Psychology Research Group, Department of Psychology, University College, London.

IN the last issue of this Journal the method of interviewing used in an inquiry in three factories (A, B and C) was described, and the effectiveness of the questions and of the control of the interviewing situation was discussed (Marriott and Denerley, 1955). The numbers of workers involved were: 340 men in factory A, 291 men in factory B and 628 men and 407 women in factory C—a total of 1,666. This inquiry was also discussed in more general terms in an earlier paper (Marriott, 1953).

THE VALIDITY OF THE METHOD

In work of this kind it is always difficult to find objective criteria with which to compare the results. Inferences about the validity of the technique can, however, be made by comparing the degree of satisfaction expressed under various physical, economic, and social conditions of work. Several examples are given below:

(a) *Level of earnings*

It is reasonable to expect that, other things being equal, satisfaction with wages would tend to increase with the amount of wages. As mentioned earlier, each worker assessed his wages on a five point scale ranging from 'very good' to 'very poor' and details were independently obtained from the factory records of his earnings in the period prior to the interview. Table 8 shows the average gross earnings (shillings per hour) of the male workers at each level of assessment. (Those with irregular earnings due to absence or other causes have been excluded.)

TABLE 8: WAGES. SELF-ASSESSMENTS RELATED TO RECORDED EARNINGS IN SHILLINGS PER HOUR

WAGES SELF-ASSESSMENT	FACTORY A		FACTORY B		FACTORY C (MEN)	
	NO. OF MEN	AVERAGE EARNINGS	NO. OF MEN	AVERAGE EARNINGS	NO. OF MEN	AVERAGE EARNINGS
Very good	128	4.34	83	3.85	75	3.89
Good	128	4.25	137	3.81	183	3.72
Moderate	30	4.06	57	3.75	238	3.46
Poor	1	3.99	1	3.50	27	3.16
Very poor	2	4.09	—	—	13	3.25
Total	289		278		536	
Factory average		4.27		3.81		3.59

In each factory, satisfaction with wages tended to be broadly associated with level of earnings in spite of the fact that the former might be expected to vary with the type and amount of work done, length of service, age, number of dependents, and by comparisons inside and outside the factory.

In factory C^x, where supplementary questions were asked, it was possible to take into account some of these variables, and in Table 9 the index of satisfaction (I.S.) with earnings has been related to size of family (children over 15 are excluded) as well as to the recorded earnings. Also included in the table are the percentages of men who said "Yes" to a supplementary question "Are your wages enough for your needs?" (E.N.)

TABLE 9: RELATION BETWEEN EARNINGS, FAMILY RESPONSIBILITIES, SATISFACTION WITH EARNINGS, AND 'WAGES ENOUGH FOR NEEDS', IN FACTORY C^X

AVERAGE EARNINGS (SHILLINGS PER HOUR)	SINGLE			MARRIED NO CHILDREN			MARRIED ONE CHILD			MARRIED SEVERAL CHILDREN		
	NO OF MEN	I.S.	% E.N.	NO OF MEN	I.S.	% E.N.	NO OF MEN	I.S.	% E.N.	NO OF MEN	I.S.	% E.N.
4.20 and above	4	1.50	100.0	19	0.79	73.7	22	0.73	59.1	22	0.55	54.6
3.80—4.19 ...	10	1.10	90.0	33	0.82	75.8	28	0.64	75.0	40	0.45	55.0
3.40—3.79 ...	23	0.70	78.3	38	0.68	68.4	40	0.33	47.5	38	0.71	52.6
Below 3.40 ...	27	0.48	74.1	53	0.30	50.9	27	0.11	44.4	28	0.04	28.6

Considering the table horizontally, it will be seen that, for each level of earnings, satisfaction with wages tended to decrease as family responsibilities increased. Similarly the percentage of men who said that their wages were enough for their needs fell as the number of persons to be supported rose. Considering the table vertically, satisfaction with wages also tended to be higher as earnings were increased whilst family responsibilities were held constant. This trend was also shown by the proportion of workers who said that they could manage on their wages. On the whole, therefore, the index of satisfaction and the question on 'wages enough for needs' reflect fairly closely the relation which it is reasonable to expect as level of earnings and the number of dependents are varied. It was not possible to take into account variations in wage expectation due to differences in skill and working conditions, both of which would tend to disturb the association. It seems probable, however, that the information obtained in the interviews gave a reasonably valid measure of satisfaction with the amount of earnings received.

(b) *Hours of work*

The five day week was introduced in each factory soon after the last war. In factory A the hours were reduced from 46½ to 42½ by cancelling the Saturday morning hours and retaining weekday hours at 8½ per day. To overcome transport difficulties, work was arranged to start at 7.30 a.m. in some departments and 8 a.m. in others. There were 10 minute rest pauses in the morning and afternoon.

The official working week in factory B was 44 hours, and on the introduction of the five day week the four hours of Saturday morning were spread over the rest of the week by beginning work three-quarters of an hour earlier, at 7.15 a.m. Thus the working day was increased from 8 to 8¾ hours. Later, owing to a local need to spread the electricity load by staggering working hours, and to shortage of supplies, daily hours were increased to 10 on four days only. One hour was allowed for lunch, but there were no official rest pauses.

The factory C hours were reduced by three per week (from 47 to 44) on the introduction of the five day week and the remaining Saturday morning hour was distributed among the weekdays by increasing the working day by half an hour, from 8½ hours to 9 hours, on four days and reducing it to 8 hours on Friday. Work commenced at 7.30 a.m. in some departments and at 8 a.m. in others, one hour was allowed for lunch and there were ten-minute rest pauses morning and afternoon.

Thus in factories A and C weekly hours had been reduced on the inception of the five day week but had remained stable since then. In factory B, on the other hand, daily hours had been considerably increased and had been very irregular owing to unsettled local and factory conditions. Attitudes to these arrangements of working hours in the three factories are shown in Table 10. Only those men who worked permanently on days are included.

TABLE 10: HOURS OF WORK. SELF-ASSESSMENTS OF PERMANENT DAY WORKERS

SELF-ASSESSMENT OF HOURS	FACTORY A		FACTORY B		FACTORY C^x	
	NO. OF MEN	%	NO. OF MEN	%	NO. OF MEN	%
Very suitable	105	60.7	23	7.9	89	58.4
Suitable	66	38.2	88	30.2	58	37.9
Neither suitable nor unsuitable	2	1.1	51	17.5	3	2.0
Unsuitable	—		107	36.8	3	2.0
Very unsuitable	—		22	7.6	—	—
Total	173	100.0	291	100.0	153	100.0
Index of satisfaction	1.60		—0.06		1.46	

Almost all the workers in factories A and C were satisfied with their hours of work, whereas only 38 per cent. of the men in factory B were satisfied. It was anticipated that the instability of working hours in factory B together with the early starting time and late finishing time would not be conducive to satisfaction and the workers' assessments confirmed this expectation.

(c) *Shift work*

Evidence from other studies (Wyatt and Marriott, 1953b) suggests that men tend to prefer regular day work to shift work in spite of the financial compensation of the latter. It seems reasonable to expect therefore that men who are obliged to accept shift work will be less satisfied than those enjoying the regularity of 'normal' day work. In factories A and C there were some men who worked on two-shift systems (there was no shift work in factory B); in factory A the change from day to night shift was made monthly and in factory C^x it was made fortnightly in some departments and weekly in others. Table 11 shows the assessments of satisfaction of the workers on permanent day shift compared with those on two shifts.

TABLE 11: SHIFT SYSTEM. SELF-ASSESSMENTS OF MEN ON PERMANENT DAYS AND TWO SHIFTS

SELF-ASSESSMENT OF SHIFT SYSTEM	FACTORY A				FACTORY C^x			
	DAYS		SHIFTS		DAYS		SHIFTS	
	NO. OF MEN	%	NO. OF MEN	%	NO. OF MEN	%	NO. OF MEN	%
Very suitable	145	83.8	16	10.7	101	67.8	37	16.0
Suitable	27	15.6	41	27.5	46	30.9	70	30.3
Neither suitable nor unsuitable	—	—	26	17.5	2	1.3	57	24.7
Unsuitable	1	0.6	46	30.9	—	—	56	24.2
Very unsuitable	—	—	20	13.4	—	—	11	4.8
Total	173	100.0	149	100.0	149	100.0	231	100.0
Index of satisfaction	1.83		−0.09		1.66		0.33	

Almost all permanent day workers were very satisfied, and in both factories A and C the shift workers were significantly less satisfied. (P is less than 0.01.)

(d) *Effects of mechanised conveyor work*

There are *a priori* grounds for expecting that those who work on mechanised conveyors are likely to be less satisfied than those doing similar jobs on non-mechanical layouts (Wyatt, 1927). There was an opportunity to test this in the two car factories (A and B) where, in the departments assembling and painting vehicles, some men worked on mechanised conveyor lines while others did not. Their assessments of satisfaction for each type of work are given in Table 12.

TABLE 12: THE OPERATION. SELF-ASSESSMENTS OF MEN COMPARED ON MECHANISED CONVEYORS AND NON-MECHANISED LINES

TYPE OF WORK	FACTORY	MECHANISED							NON-MECHANISED						
		SATISFACTION*					NO. MEN	I.S.	SATISFACTION*					NO. MEN	I.S.
		VS	S	N	D	VD			VS	S	N	D	VD		
Chassis assembling	A	1	2	0	2	1	6	.00	2	5	2	1	0	10	.80
	B	1	11	10	1	0	23	.52	4	7	1	0	0	12	1.25
Painting ...	A	2	1	3	0	0	6	.83	3	6	4	1	0	14	.79
	B	11	17	9	8	0	45	.69	4	0	2	0	0	6	1.33
Body work ...	A	3	7	6	2	0	18	.61	12	19	4	2	0	37	1.11
	B	17	25	33	12	3	90	.46	16	25	13	7	0	59	.85
Total ...	A	6	10	9	4	1	30	.53	17	30	10	4	0	61	.98
	B	29	53	52	21	3	158	.53	24	32	14	7	0	77	.95

*VS—very satisfied. S—satisfied. N—neither S nor D. D—dissatisfied. VD—very dissatisfied. I.S.—index of satisfaction (see Part I).

In every comparison except one, the workers on mechanised conveyors tended to be less satisfied than those on other lines, and the similarity between the two factories is strikingly close. Although it is not claimed that the conveyor and non-conveyor jobs were identical they were as similar within each type of work as is likely to be found in an industrial situation. It therefore seems probable that the different levels of satisfaction shown by the self-assessments can be attributed to the effects of these working conditions. Examination of the detailed likes and dislikes expressed by the workers supports this view. Table 13 provides an illustration of this. It shows the proportion of men in factories A and B who spoke spontaneously of the interest or monotony of their work. The workers on non-mechanised layouts have been separated into those on non-conveyor 'line' work and on individual benches and machines. (This table is reproduced from Wyatt and Marriott, 1953a.)

TABLE 13: NUMBER AND PERCENTAGE OF MEN ON MECHANISED AND NON-MECHANISED LAYOUTS WHO SPOKE ABOUT INTEREST OR MONOTONY

	MECHANISED				NON-MECHANISED							
					LINE WORK				INDIVIDUAL WORK			
	FACTORY A (43 MEN)		FACTORY B (169 MEN)		FACTORY A (195 MEN		FACTORY B (30 MEN)		FACTORY A (102 MEN)		FACTORY B (18 MEN)	
	NO.	%	NO.	%	NO.	%	NO.	%	NO.	%	NO.	%
Interest	15	34.9	58	34.3	109	55.9	17	56.7	68	66.7	17	94.4
Monotony ...	23	53.5	93	55.0	82	42.1	11	36.7	40	39.2	5	27.8

These results indicate that the men on mechanised lines were less interested and more bored than those on non-mechanised layouts and that individual work at the bench or machine was rather more interesting than 'line' work where the component was passed from one man to the next. The tendencies in both factories were similar and in agreement with those in Table 12. In both cases it can be claimed they are in line with expectation.

(e) *Size of group and payment system*

In answering questions on the group payment systems in factories A and B, some workers made *spontaneous* references to the size of groups about which no questions were asked, and there was a decided preference for small groups. The supervisors were asked *direct* questions about the size of groups and they also definitely favoured small groups. The distribution of the comments was as follows (F—favourable, U—unfavourable).

	NO. OF WORKERS				NO. OF SUPERVISORS			
	FACTORY A		FACTORY B		FACTORY A		FACTORY B	
	F	U	F	U	F	U	F	U
Small groups ...	20	0	5	1	40	14	22	1
Large groups ...	2	12	1	3	10	38	7	8

These results led to a study in both factories of the effects of group size on output and a significant inverse relationship was established (Marriott, 1949). All the production workers were included in this part of the inquiry, not only those interviewed (10% in factory A and 20% in factory B). With the consistent decrease in output as the size of the groups increased, it was expected that the attitudes of the workers would also show some relationship to group size despite the absence of questions on the subject. Analysis of the data showed that this was present only in the case of the payment systems which were, of course, based on the working groups and, as the above spontaneous replies indicate, were closely associated with them in some of the workers' minds. Table 14 shows the similar trends in each factory.

TABLE 14: PAYMENT SYSTEMS. SELF-ASSESSMENTS SHOWING THE RELATION TO GROUP SIZE

GROUP SIZE	FACTORY A							FACTORY B						
	VS	S	N	D	VD	NO. MEN	I.S.	VS	S	N	D	VD	NO. MEN	I.S.
0—19	37	18	4	16	5	80	0.83	41	21	6	13	0	81	1.11
20—39	42	44	9	23	5	123	0.77	10	13	1	4	0	28	1.04
40—59	14	19	5	16	2	56	0.48	17	13	4	5	3	42	0.86
60—79	11	6	1	9	2	29	0.52	9	11	2	4	4	30	0.57
80—99	5	3	0	11	1	20	0.00	3	8	1	8	2	22	0.09
100 and over	8	7	2	12	3	32	0.16	5	4	1	4	0	14	0.71
Totals	117	97	21	87	18	340	0.61	85	70	15	38	9	217	0.85

With the exception of the largest groups and one slight rise in group 60-79 (factory A), the table shows consistently decreasing indices of satisfaction with the payment system as the size of the group increased. The upward trend in the largest groups was also present in the study of output in relation to group size.

(f) *Payment system*

Another interesting trend shown in Table 14 is the greater general satisfaction with the payment system in factory B as compared with factory A. This attitude is consistently reflected at each group size as well as in the totals. Though the difference in Table 14 is not significant (P is greater than .10), it is when the attitudes of workers on similar types of work in the two factories are compared:

FACTORY	VS	S	N	D	VD	NO. MEN	I.S.
A	31	28	9	37	10	115	0.29
B	58	36	7	17	8	126	0.94

(P is less than 0.01)

It is significant that only in the topic of payment systems were the workers in factory A less satisfied than those in factory B. The factory A system was a percentage bonus based on the ratio of the time allowed to the time taken to carry out the operations. Calculation of the bonus was very involved and it was practically impossible for most workers to judge their output or earnings until the basic information in the form of an 'efficiency percentage' was supplied by the Wages Department at the end of the week. Even then, except in the simplest operating groups, the individual worker was unable to assess his contribution to the group effort. In factory B the system was group piece work expressed in terms of money for each operation. In almost every case the workers were able to keep track of their output and earnings at any stage of the day and week and to relate their effort to that of the group.

Thus the system in factory B was much simpler and the workers had a greater knowledge of the results of their efforts which was largely absent in factory A. In view of the insistence by almost all writers on incentive payment systems (*e.g.*, Viteles, 1954) on the need for simplicity, and of the research done on the effect of knowledge of results in increasing interest and satisfaction (Wyatt, 1934; Mace, 1935), it was expected that factory B would show a higher level of satisfaction than factory A.

Among other factors which probably contributed to the greater satisfaction with the group payment system in factory B were the more numerous conveyor operations than in factory A. One of the most frequent complaints in group work was of 'slacking' by fellow workers. On conveyor work, allotted tasks have to be completed before a component passes by and there is no opportunity for anyone to 'slack' without it becoming immediately apparent to all. Thus, one of the causes of poor worker relations – suspicion of slacking, whether justified or not – was largely removed.

These examples of the relation between attitudes and such objective criteria as level of earnings, family responsibilities, hours of work, shift system, type of work, size of group and payment systems suggest that on these topics this method of interviewing had a reasonable degree of validity.

(g) *Management policy*

It is, of course, more difficult to provide evidence of the relation between satisfaction and the social aspects of the working situation. The effects of physical and economic factors are usually compounded with those of the less tangible factors of human relations. In comparing the two car factories (A and B) it was, however, possible to control certain of these variables. Physical environments, type of work and recreation facilities were generally so similar that minor differences between them were unlikely to account for any difference in attitude towards the firm. In managerial policy there were, however, obvious and subtle differences which were apparent to the impartial observer. Comparison of the two firms in dealing with similar situations in previous years showed quite marked differences, for example, in the methods of introducing the five day week (1946); in the generosity of treatment during the three week closure necessitated by the fuel crisis (1947); in the arrangement of hours of work as described above (p. 71); in the level of earnings compared with wages paid by other firms in the same district; and in the personal participation by higher management in the practice of joint consultation. These and other differences have been described elsewhere (Marriott, 1951).

The general expectation from this evidence was that the workers in factory A would be more satisfied with the firm than those in factory B.

After stating quite freely their likes and dislikes concerning all aspects of the working environment, the men were asked to assess their attitudes on a scale ranging from "I like working here very much" to "I dislike working here very much". Table 15 is a summary of the results.

TABLE 15: ATTITUDES TO THE FIRM AND GENERAL WORKING SITUATION

	FACTORY A		FACTORY B	
	NO.	%	NO.	%
Very satisfied	177	52.1	69	31.8
Satisfied	136	40.0	102	47.0
Neither	18	5.3	23	10.6
Dissatisfied	8	2.4	21	9.7
Very dissatisfied	1	0.3	2	0.9
Total	340	100.1	217	100.0
Index of satisfaction	1.41		0.99	

The difference is significant (P is less than 0.01) and a similar difference was shown in the replies to the preceding question in which workers were asked what were the main things they liked "about working here". In factory A management was spontaneously referred to by 57% of the workers while in factory B only 17% did so. Workers were also asked to whom they gave credit for the things which they liked. In factory A 77% gave credit to the management but in factory B only 48% did so. Approval of the joint consultative machinery was expressed by 83% in factory A and by 52% in factory B. Thus the attitudes which were expressed in the interviews conform to the pattern which would be expected from the records of management policies in factories A and B. Because of the additional variables involved it is not possible to make any such direct comparisons with factory C.

Thus on all topics where it has been possible to make predictions the attitudes expressed by workers are consistent with expectation. In the absence of more objective and indirect criteria it is therefore suggested that this method of interviewing has a reasonable degree of validity.

DISCUSSION

The testing of interviewing methods was not the primary aim of this inquiry into workers' satisfaction and discontent, and experiments in technique were necessarily limited by the time and facilities available. It was, therefore, decided to choose a method developed from much previous experience in the use of similar questions, and, after initial trials, to follow it through as carefully as circumstances allowed. The new features introduced were largely in the re-arrangement of the questions and in the use of similar types of questions grouped under specified 'topics' which previous inquiries had shown to include most of the workers' views about the factory situation. It was hoped that in covering these major aspects of working life the interview results would differentiate between the broad effects of various social, economic and physical conditions, and enable the investigator to assess and test them against observation and any objective criteria available.

The method was used essentially as an instrument for assessing the collective attitudes of individuals composing the various groups under observation and not as a tool for measuring individual attitudes as such. In this way it was felt that the influence of intangible and inaccessible variables would at least be diminished, if not 'smoothed out'. This did not mean any lessening of the care taken to put individual workers at their ease by preparatory procedures directed at them and the groups before and during the interviews. It did mean, however, that individual differences in fluency, ability to appreciate the work situation and to assess their attitudes to it were reduced in importance.

In addition to the aims of the method, as stated in the introduction—to give the worker freedom to express his views and assess his attitudes to the whole working situation—it was hoped that the basic questions would provide a simple foundation on which to expand or restrict the interview. (An illustration of this was provided in factory CY where the number of topics was reduced so as to concentrate mainly on the problem of the payment system, that section being expanded by the inclusion of more subsidiary questions.)

The basic questions on the workers' likes and dislikes for each topic under discussion, the self-assessment of his attitude on a five-point scale and a further check question on the reasons for his choice formed the framework of the interview. Such questions are simple, direct and unambiguous, and by their juxtaposition and the order in which they were presented have the additional merit of both stimulating thought and providing an opportunity to correct any bias shown. Workers not infrequently qualified or emphasised their first statements by giving reasons for a self-assessment which was not quite in line with previously expressed likes and dislikes; in general, however, it was a matter of confirming them. The value of this procedure was also in the lack of dependence on single questions and in the time and stimulus to concentrate on one particular aspect of working life though there was no restriction put on a worker who wished to widen the scope of the discussion. Tables 2 and 3 have demonstrated that there was a close relationship between the results obtained from the different types of questions on each topic, and Table 4 has shown that it remained constant for the different interviewers. This consistency is some indication of the reliability of the method and of the effectiveness of the questions. The view that more than one question on each topic is necessary has also been put forward by Heron (1954) who suggests that the questionnaire and interview methods should be combined "in order to maximise the advantages and minimise the drawbacks of them both". He also agrees to the separate treatment of each topic (or item) and states that it "is in any event likely to reduce halo effect by separating in time items similar in content and by focussing the subject's attention on each item in visual isolation from all others".

It may be argued with some justice that the manual worker is not trained to analyse his feelings or the situation in which he works and that this renders him the more liable to distort his views and to displace his emotions. It should be remembered, however, that (1) the interview was dealing with a distinct and restricted portion of the worker's life—distinct in so far as it is physically separated from home and leisure life; (2) the worker is constantly and directly stimulated by his working environment, discusses it frequently with his workmates and is, on the average, fairly articulate about it, especially if the interview technique has been designed to assist him in this respect; and (3) the object was to assess the average attitude of the group, not that of the individual.

A further aim of the method was to restrict the influence of the interviewers who were instructed not to deviate from the questions. Much has been written about the influence of interviewers and ways of reducing it (*e.g.*, Wyatt and Campbell, 1950; Silcock, 1950; Ferber and Wales, 1952). Control seems to be especially necessary in the face to face situation with a manual worker where the apparent or real status and personality of the interviewer are perhaps most strongly asserted. Such control need affect neither the smooth running of the interview nor the worker's freedom, but it gives the interviewer time to concentrate on an accurate and fuller record and assures as nearly as possible the same situation for each worker. It is not proposed to enter here into a controversial discussion concerning so-called 'free' and 'controlled' interviewing, for the verdict as to which is the more desirable awaits much needed experimental research. At present the choice of method rests on the views of the investigator who has to take into account the nature of the inquiry and the type of situation. Whichever method is used, it seems probable that the reliability of the results will be reduced unless some check is possible of the impact of the interviewer on the interviewee. The evidence given in Tables 5 to 7 based on the results of several interviewers suggests that in this investigation their influence was reduced to a minimum by the method employed.

The claim that this method as applied to groups of workers does succeed to some extent in measuring what it purports to measure is supported by the evidence given in Tables 8 to 15. Although the data do not provide as high a degree of objectivity as is desirable, the evidence does support the claim that this method of interviewing gives a broad measure of the average worker's attitude to certain material and social conditions. Moreover, the results obtained by several interviewers from similar samples showed close agreement and similar results were obtained from the three independent factories. What is lacking in objectivity is therefore partly counterbalanced by interviewer and factory repetition.

It can reasonably be claimed that attitudes affect behaviour and any method which succeeds in measuring them to a more accurate degree than common sense or ordinary observation is of value. Allowing for all its imperfections, there is little doubt that a well constructed and administered interview will do at least this. Rodger (1952) in a general appraisal of the value of the interview agrees with the views of Vernon and Parry (1949) that "although thorough psychometric procedures might usually be more accurate . . . they may be impracticable in addition to being unacceptable" and "the interview has the tremendous advantage of inclusiveness, speed and flexibility". Rodger concludes that "the main task of the psychologist in this field . . . is not to disparage the interview but to improve it". It has been one aim in presenting this paper to encourage more discussion of the problem.

SUMMARY (PARTS I AND II)

1. This paper gives an account of the methods used in interviewing 1,666 production workers in three factories. Sampling procedures and methods of analysing the data are also described.
2. The discussion of the method is centred mainly on two types of questions which formed the framework of each section of the interview. These sections dealt with eight topics which previous experience had shown to cover most of the workers' views of the factory situation; there was also one on general satisfaction.
3. The two types of questions were concerned with the workers' likes and dislikes for each topic and the assessments of their attitudes on a five-point self-rating scale. It is shown that the replies to these questions are related and that the relationship is consistent for each topic in each factory and for each interviewer.
4. Comparison of the attitudes of groups of workers at the beginning and end of the interviewing periods shows very little effect resulting from the continued presence of the investigators, the influence of the interviewer on the workers and of the workers on each other.
5. The validity of the method is discussed in terms of the relationship between satisfaction as shown by the self-assessments and such variables as level of earnings, family responsibilities, hours of work, shift work, mechanised and non-mechanised work, size of group, payment system and management policy.

The authors gratefully acknowledge the advice received from Dr S. Wyatt, Mr J. W. Whitfield and Dr P. Armitage, the help of their colleagues and the co-operation of the workers, supervisors and managers who took part in this investigation.

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Experiments on the Acquisition of Industrial Skills (Part 2)

By W. DOUGLAS SEYMOUR

Mr Seymour is continuing his study of the nature and acquisition of industrial skills as University Research Fellow (financed by Conditional Aid funds) in the Department of Engineering Production at Birmingham. This is an abridged report.

THE first part of this report (Seymour, 1954a) dealt only with the results obtained in the final fifteen-minute test runs after a week's practice at a task on a capstan lathe. The following article discusses the more detailed results which were obtained by timing and recording each part or element of the operation throughout each subject's learning period.

SOURCE OF THE INFORMATION

As the subject completed, in turn, the elements pick up and insert component, lock collet, switch on, and operate cross slide, etc., on the capstan, contacts were made and electrical circuits operated a Kymograph tape recorder. Measurement of the tape provided, for each trial of each subject, not only the total time per cycle of the operation, but also the element times contributing to each total. There were, in all, some 40,000 of these individual element times in the experiments so far reported, and the results have been transferred to graphs so that the progress of the acquisition of skill on the task could be studied.

Only four of these graphs are included in the present article, as the reproduction of the remainder is not at present feasible. The outstanding features of the groups, and of the tables of figures on which they are based, are therefore discussed in summary form.

GENERAL OBSERVATIONS

Although, as mentioned in the previous article (Seymour 1954a), there was no significant difference between the final test-run results of part and whole methods in the first half of the experiment, whereas there was such a difference in the second half, it appears from the shape of the curves (cf. Figs. 1 and 2) that even in the first half the manner in which the skill (to the same level of performance) was built up by the whole method was different from that when part methods were used. Again, it is evident (cf. Figs. 1 and 2 with 3 and 4) that although the sequence of movements, and the movements themselves, in the two halves of the experiment were the same*, the level of performance on similar elements did not necessarily recommence in the second half at the point it had left off in the first, nor did the skills develop in the same way in the latter half (where a higher perceptual stringency was involved) as in the former.

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*There was no difference in 'pattern of movement' but there was a change in the manipulation involved in grasping the component and locating it in the collet. This change occurred owing to the different shape of the component in the second part of the experiment.

OBSERVATIONS ON THE FIRST HALF

(a) *Using the Whole Method*

Fig. 1 shows the curves of improvement for only one subject, R.P., using the whole method, on the first half of the experiment (i.e., where movement was involved, but little perceptual stringency), but this graph is typical. The shape of the top curve—the total time per cycle—follows the pattern of a normal learning curve and shows considerable improvement over the course of the practice. There are several peaks, most of which coincide with similar peaks in the curve of turret time, which comes immediately below it. The best performance was attained at the end of the penultimate session. The times on the final test run are not quite so good, and this is due perhaps to the fact that it *was* a test run, or to slight fatigue effects. The shapes of the element curves are generally similar. That for the turret time follows a pattern almost identical with that of total time, and takes a similar length of time to attain a fairly steady level. The shorter elements, though they show a rapid initial improvement, level off after a smaller number of repetitions. Unfortunately the recordings of the first twenty cycles for elements pick up, insert and close collet and place aside, are missing. The first of these appears to have approached close to the steady level as soon as recordings commence, but the shapes of the other two suggest that they followed the same pattern as the other short elements.

It is interesting to observe where the improvement in total time comes from, and how much arises from each element of the task. The sources and percentage of improvement are indicated in Table I below, which shows the totals of the first ten complete recordings for each subject on the first day and the last ten complete recordings on the final test run.

TABLE I: IMPROVEMENTS IN ELEMENT TIMES, IN SECONDS, FIRST HALF, WHOLE METHOD, FOR SEVEN SUBJECTS

ELEMENT	V.P.			R.P.			B.T.			S.K.			M.C.			J.E.			J.G.			TOTAL OF DIFFER-ENCES.	%AGE TOTAL DIFFER-ENCE
	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.	TOTALS FIRST 10	TOTALS LAST 10	DIFF.		
Pick up ..	7.6	5.2	2.4	7.6	3.6	4.0	5.7	4.4	1.3	6.0	3.6	2.4	8.1	6.3	1.8	7.3	5.1	2.2	6.2	2.6	3.6	17.7	3.7
Insert and close collet	14.6	9.9	4.7	18.9	13.1	5.8	12.9	7.3	5.6	18.7	11.8	6.9	24.4	11.3	13.1	15.3	9.2	6.1	14.8	9.3	5.5	47.7	10.0
Switch on	5.7	3.8	1.9	7.3	5.0	2.3	6.5	2.6	3.9	10.8	4.4	6.4	10.7	4.2	6.5	9.4	5.1	4.3	8.6	3.8	4.8	30.1	6.3
Front C/s	24.0	18.8	5.2	22.7	22.2	0.5	21.3	18.2	3.1	26.1	29.6	-3.5	26.8	21.6	5.2	26.8	21.0	5.8	26.1	23.3	2.8	19.1	4.0
Back C/s	24.2	20.6	3.6	22.8	20.1	2.7	20.8	16.6	4.2	27.1	23.5	3.6	26.5	21.5	5.0	26.6	20.5	6.1	28.2	19.8	8.4	33.6	7.0
Change Speed	5.5	2.7	2.8	11.3	7.1	4.2	7.4	1.0	6.4	5.6	2.0	3.6	9.9	1.9	8.0	7.9	1.3	6.6	9.4	5.8	3.6	35.2	7.4
Turret ..	71.1	47.4	23.7	73.9	51.4	22.5	70.7	44.4	26.3	81.5	56.2	25.3	74.1	54.1	20.0	83.6	50.9	32.7	84.7	46.2	38.5	189.0	39.6
Switch off	13.2	7.8	5.4	15.3	10.4	4.9	12.7	8.9	3.8	14.5	9.4	5.1	17.8	5.1	12.7	13.6	5.4	8.2	14.7	7.4	7.3	47.4	9.9
Remove ..	8.9	6.2	2.7	8.5	7.6	0.9	8.3	4.7	3.6	11.4	4.9	6.5	10.1	6.6	3.5	8.6	8.2	0.4	9.5	7.7	1.8	19.4	4.1
Place Aside	12.8	5.9	6.9	9.9	6.6	3.3	11.4	6.8	4.0	13.5	6.0	7.5	22.4	11.7	10.7	10.3	8.4	1.9	8.9	5.3	3.6	38.5	8.0

The relation of curves of element improvement to those of total cycle improvement is roughly similar to that found by Barnes (1940) in the study of therblig times in an assembly operation, though in his case some therbligs were 'inside cycle' time of others and did not follow the same pattern of improvement. Again, as in Barnes's results, there are no noticeable plateaux in the learning curves, either for the total cycle time or for the elements.

(*b*) *Using the Part Method*

Fig. 2 shows the curve of improvement of subject C.M. using the part method. The first notable feature of the graphs of this group of subjects learning by the part method is that the curves show little improvement, and that the time taken, both for individual elements and for the total cycle, is very little reduced as practice proceeds. The curves tend toward horizontal straight lines with variations up and down, but final times are hardly ever shorter than initial times. This is exemplified by the times for completing the cross slide movements, where the over-all improvement is approximately 4.5%. The corresponding improvement shown by subjects on the whole method was from 362.5 to 299.5, i.e. 17.5%. The difference between the means of the differences is barely significant—P is between the .005 and 0.10 levels. Omitting subject D.M., the difference in means is significant, P being between 0.1% and 1.0%.

Next, when we examine the initial total cycle times for these subjects who had done the elements, both singly and in pairs, before attempting the whole job, it is apparent that their 'first attempts' are very close to target time. This is shown in Table 2 which gives the first five recorded times for the total cycle of the operation for each of the seven subjects, using the part method.

TABLE 2: TIME IN SECONDS FOR FIRST FIVE ATTEMPTS, TOTAL CYCLE, PART METHOD, FOR SEVEN SUBJECTS

SUBJECT	1ST	2ND	3RD	4TH	5TH
D.G.	15.1	13.8	13.2	14.3	13.5
C.M.	13.4	14.1	12.4	11.6	13.7
L.B.	16.6	15.0	14.8	14.2	12.6
P.B.	14.7	14.2	14.3	14.7	15.4
J.J.	17.3	—	15.0	16.2	14.1
D.McC.	17.9	21.6	14.7	15.5	14.0
D.M.	13.0	—	15.7	12.5	13.5

The comparison of these times, and of the elements comprising them, with those element times which had been attained by previous practice on the elements alone, indicate that there is a transfer from the practice on the parts and would tend to confirm the findings of Gagne (1947). This is exemplified by the times recorded for operating the turret. A time less than target was usually attained immediately when the element was attempted by itself, and there was little change when the element was repeated in combination with the cross-slide and, later, as part of the whole cycle. There thus appears to be a considerable measure of transfer of skill from the individual or component parts to the whole task.

(c) *Comparison of Part and Whole Method Results*

Comparing the curves for subjects using a part method with those of subjects using a whole method, it would appear that, *although the final test results are not significantly different* (Seymour 1954a) *the way in which the final level of skill was attained is different.* In particular, it appears that the number of repetitions required to attain a given target level of speed on a component element of a task is greater when that element is practised as part of the total task than when it is practised on its own. This is exemplified by the comparison of the number of repetitions of the turret element by subjects using part and whole methods respectively before the target time (6 secs.) was attained. Whereas those using the part method attained target at the first attempt, (although one subject did not consistently maintain it till after the third attempt), those using the whole method made 21, 58, 15, 53, 51, 38 and 45 attempts respectively before target time was reached.

A study of the corresponding figures for other elements reveals the same point, namely, that the target of experienced workers' speed can be attained with appreciably less practice for any element of the task when that element is practised independently instead of as a part of the total cycle.

OBSERVATIONS ON THE SECOND HALF

When we turn to the graphs and results of the second half of the experiment in which, so to speak, the 'skill' of the capstan operation was introduced, and the perceptual stringency of certain elements simulated what it would have been in a real operation, we find a quite different picture. Although the over-all pattern of movement of the component, of the hands, and of the machine controls was identical, the rate of movement on the front cross-slide tool and on each of three turret tools (in alternate stations) was restricted to what it would be in a typical operation, and a red warning light flashed if these elements were wrongly performed, i.e., done in such a way as would produce scrap in the factory.

The final results in this latter half of the experiment have already been described (Seymour, 1954a).

In this part of the experiment, the results have to be recorded in two ways, in terms of quantity (or time) and quality (or errors). It may be noted that this complication of the scoring becomes inevitable as soon as an appreciable degree of control of movement—or skill—is introduced into a task. In the present experiment, scores have been quoted in terms of "clear runs", "turret errors" and "cross slide errors" for the final test run, in which it was found that the results from the part method were superior to those from the whole method, both in terms of more clear runs and of fewer errors.

Transfer—About four months elapsed between the two halves of the experiment, and in view of the similarity of movement pattern and dissimilarity in perceptual stringency of certain elements, it would have been very useful to have determined the amount of transfer of skill from the first to the second half of the experiment. Complicating factors have, however, made it difficult to isolate the transfer effect. First, because some elements, e.g. front cross slide and turret, although involving identical movement patterns in both halves, were perceptually so changed that in the second half their target time was increased by 4 seconds and 24 seconds respectively. Thus, as measured in terms of time, it is difficult to indicate how much transfer there was from a previous element taking less time. Secondly, because those subjects who did the whole method on the first half of the experiment used a part method on the second, and vice versa. As was pointed out above, an element done in isolation initially takes much less time than the same element in combination so that the method of operation used, and change in method, masks any transfer effect.

Thus, even where an element was identical in both halves of the experiment, e.g. back cross slide, it is difficult to determine to what extent the time taken for initial attempts in the second half is due to transfer effect, and to what extent due to the method (part or whole) used. A careful scrutiny has been made of the times for the first ten and last ten operations of the cross slide in each half of the experiment, and superficially there would appear to be little evidence of transfer of skill on this identical element from the first to the second half of the experiment.

With the exception of S.K., who took 0.6 secs. less, all those who changed from whole to part method took longer on the element when they restarted than when they finished the first half; but their times when restarting are all less than their original times. Those who went from part to whole method on the other hand, in most cases took longer to perform the element when restarting than when finishing the first half; but *all* took longer on restarting than when doing the element initially at their first attempts on the first half. The amount of improvement in the second half is less with the part method than with the whole method, though there is no significant difference between the final scores.

The following points concerning transfer of skill are suggested by a consideration of the figures which, however, fail to provide any precise information on the subject. First, the time taken to perform an element when repeated in a different context may depend upon the nature of that context, i.e. the perceptual and spatial character of adjacent elements. Secondly, the time may be affected also by the method by which the task is learned, i.e. a part or whole method. This may also apply retrospectively, i.e. time may be affected by the way in which the element was originally learned. Thirdly, the amount of transfer may vary from subject to subject, and subjects may react differently to different methods of training (see Seymour, 1954a).

(a) *Using the Whole Method*

The detailed results of those using the whole method on this half of the experiment follow a much less clear pattern than those from the first half. The sequence of operations was the same, and this was remembered by all subjects and presented little difficulty. Times for the total cycle and for certain individual elements are, however, extremely variable. The elements which were identical e.g. switch off, switch on, place aside, were more variable, but after erratic changes settled down to times approximating to those in the first half. The only real difficulties were those experienced in the elements which had changed. Of these, pick up caused a little initial trouble, as the component was now circular instead of cylindrical. But the major difficulties were those encountered in front cross-slide and turret, which involved exactly the same motions as before, but the movement had to be controlled to give a finely discriminated rate of application of pressure. Failure to achieve this on either of these two elements not only "scrapped" the particular cycle, but also affected the way in which a subject attempted the next cycle. As has been so often observed in industry, failure to achieve success on a particularly difficult element slows down the pace on the succeeding cycles, and affects the speed at which other easier elements in the cycle are performed.

The graph Fig. 3 shows the results of subject D. McC. whose final performance was nearest to the average. The element times for pick up were not recorded in many cycles, owing to failure of a switch, and the times for these and for the next element have been omitted from the graph. Total time is initially extremely erratic and the variations arise from large changes in turret time. Failures on operating the turret in one cycle led to very cautious attempts in succeeding cycles, and the earliest recorded successes on this element took three and four times the target time, which was not achieved until the third day. It may be noted that three of the seven subjects did not achieve target time on the turret even on the fourth day, and another achieved it only once.

The front cross-slide presented similar but somewhat less difficulty. The curve for the element on subject D. McC's graph shows at first a fairly steady improvement, beginning well above the target but coming down below target level before the end of the first day. On the second day it started again above target and came down to about the same figure. Thereafter it remained fairly steady as though a plateau had been attained.

(b) *Using the Part Method*

Fig. 4 shows the graphs of subject S.K. whose results came nearest to the average of the group. The early results on the part pick up, insert and place aside are very erratic due to fumbling, but after seven attempts the target was achieved and thereafter maintained. The front and back cross slide was similarly erratic at the commencement but improved rapidly. The total of the first ten successful operations of the front cross slide was 65 secs., of the last ten 43 secs. It may be noted that the corresponding figures for the back cross slide were 24 secs. and 17 secs. The figures for the other six members of this group were similar for the front, though their improvement on the back cross slide was less marked.

The turret, even though attempted by itself, presented considerable difficulty. Only two successful attempts were recorded on the first day, and these took approximately twice the target time. Practice on the second day showed rapid improvement and times of 10% over target were attained; it was not until the third day, however, and after 17 successful attempts, that target time was reached and maintained. The corresponding results for the other six subjects in this group are shown in Table 4.

It is thus clear that the immediate attainment of target which was achieved by the isolation of elements in the first half of the experiment, did not recur in the second half, wherein the perceptual stringency of the turret element was much greater, though the movement identical.

A point of considerable importance in part methods of training is the effect on element times of combining one part with another. It will be seen from the graph Fig. 4 that when the cross slide is first combined with the insertion of the component, there is a very slight temporary rise in front cross slide times; when this combination is repeated (on the 3rd day) no increase occurs. Again, when the turret is first combined with the cross slide there is a slight rise, the four attempts before combination totalling 141 secs., the four after combination totalling 154 secs. When the combination was repeated on the next day, the corresponding figures were before combination 130 secs., after combination 122 secs.

(c) *Comparison of Part and Whole Method Results*

The test run results for this part of the experiment have already been given (Seymour 1954a) under the headings of 'clear runs', 'turret errors' and 'cross slide errors'. It is now possible to compare the results in terms of mean time per clear run. Table 3 shows the average of the recorded total times for each of the two groups during the final test run:—

TABLE 3: MEAN TIME FOR A CLEAR RUN IN FINAL TEST

	WHOLE METHOD	PART METHOD
Mean	47.12	42.17
sigma2	145.03	24.80

F = 5.85 (significant)

t = 2.304 (modified test as F is significant)

d.f. = 166

5% < P < 2%

It will be seen that the times recorded by subjects on the part method are slightly less than those from whole method subjects, but in addition their times are very much less variable. The difference in average time is only 4·95 secs.—some 10%—but the difference in variability is of the order of 2 : 1.

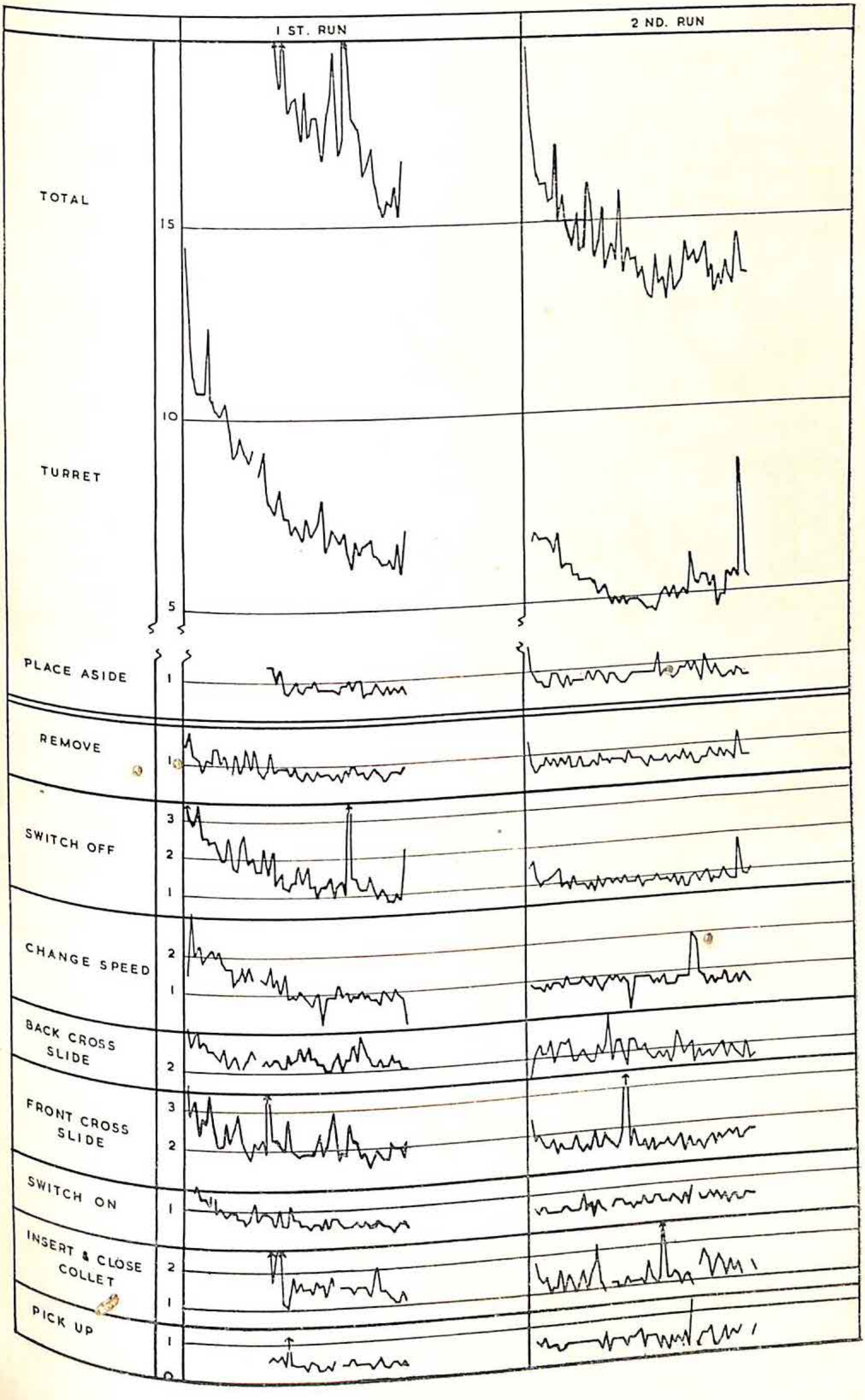
Next, it is interesting to compare the number of attempts required to attain target speed on an element of the task when done in isolation with that required when the task is done as a whole. The figures given below in Table 4 are the number of attempts at the turret element, which proved the most difficult to all subjects, before attaining target time without error.

TABLE 4: NUMBER OF PRACTICES TO ATTAIN TURRET TARGETS IN SECOND HALF OF EXPERIMENT

PART METHOD		WHOLE METHOD	
SUBJECT	NUMERICAL ORDER OF ATTEMPT ON WHICH TARGET ATTAINED	SUBJECT	NUMERICAL ORDER OF ATTEMPT ON WHICH TARGET ATTAINED
V.P.	30th attempt	D.G.	not attained (52 attempts)
R.P.	13th attempt	C.M.	15th attempt
B.T.	23rd attempt	L.B.	not attained (67 attempts)
S.K.	17th attempt	P.B.	23rd attempt
M.C.	10th attempt	J.J.	30th attempt
J.E.	6th attempt	D.Mc.	35th attempt
J.G.	7th attempt	D.M.	not attained (77 attempts)

It will be seen that although this element, even in isolation, required many more repetitions before target was reached than in the first half of the experiment, still more repetitions were required when the operation was done as a whole. It may be noted that, as stated in the previous article (Seymour, 1954a), the pressure required for each of the three tools in the turret was different. *All* the turret tools were practised together in the part method; different and perhaps better results would have been attained if each tool had been practised separately before even the turret element as a whole was attempted.

FIG. 1



1 ST. RUN
2 ND. RUN
TOTAL
15
10
TURRET
5
PLACE ASIDE
1
REMOVE
1
SWITCH OFF
3
2
1
CHANGE SPEED
2
1
BACK CROSS SLIDE
2
FRONT CROSS SLIDE
3
2
SWITCH ON
1
INSERT & CLOSE COLLET
2
1
PICK UP
1
0

FIG. 1—*continued*

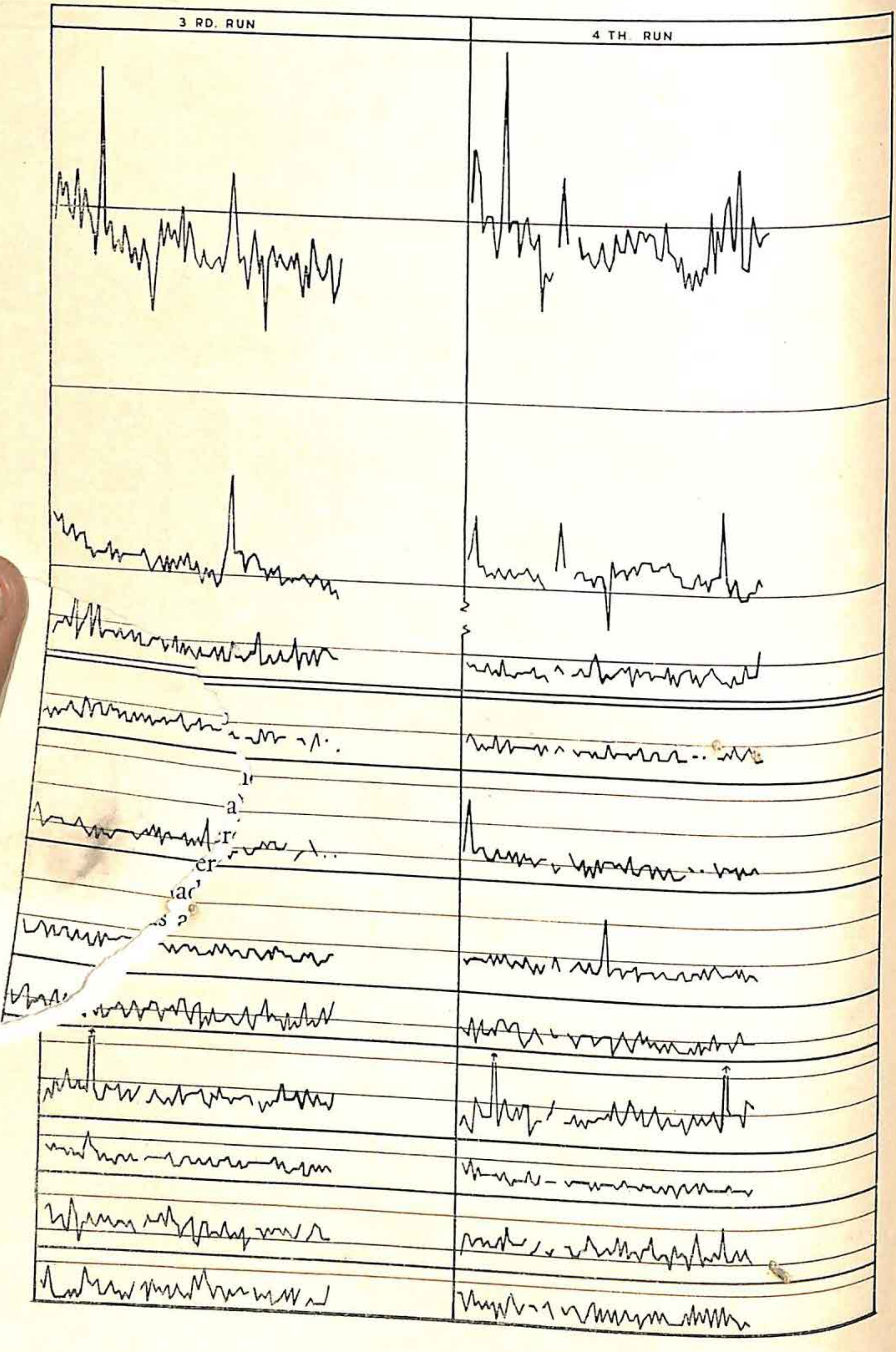


FIG. 2

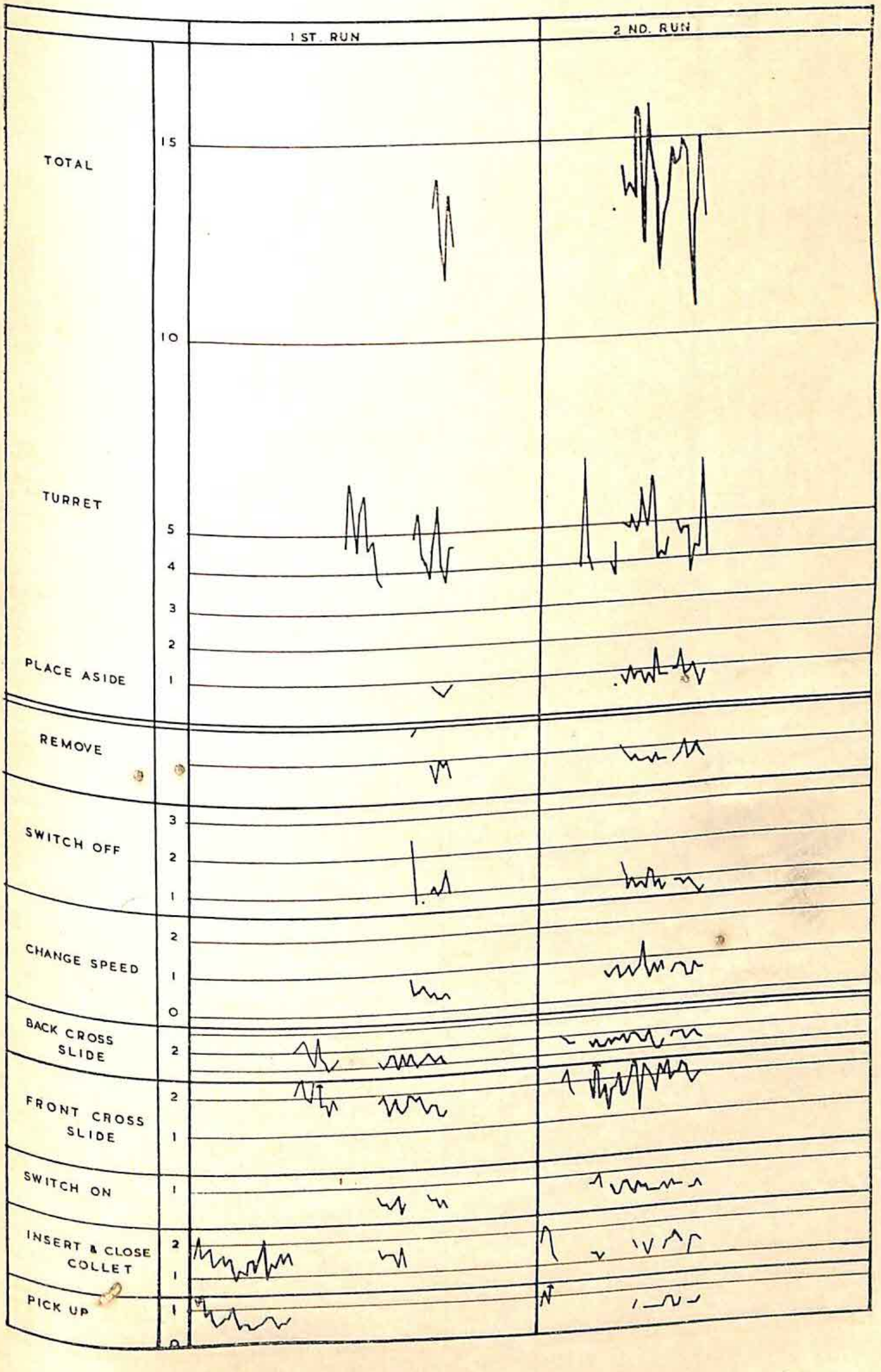

1 ST. RUN
2 ND. RUN
TOTAL
TURRET
PLACE ASIDE
REMOVE
SWITCH OFF
CHANGE SPEED
BACK CROSS SLIDE
FRONT CROSS SLIDE
SWITCH ON
INSERT & CLOSE COLLET
PICK UP
15
10
5
4
3
2
1
3
2
1
2
1
0
2
2
1
1
2
1
1
0

FIG. 2—*continued*

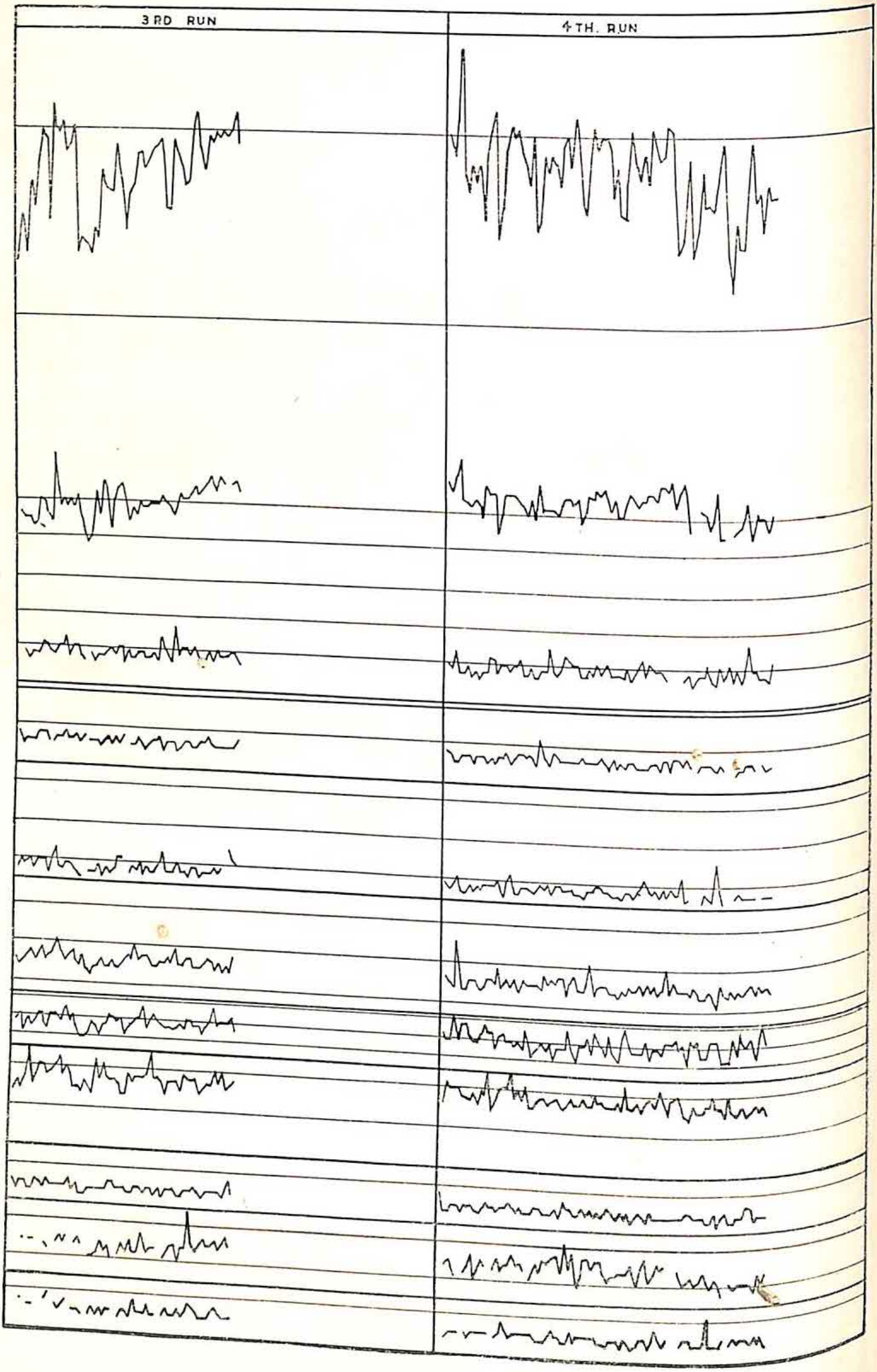


FIG. 3

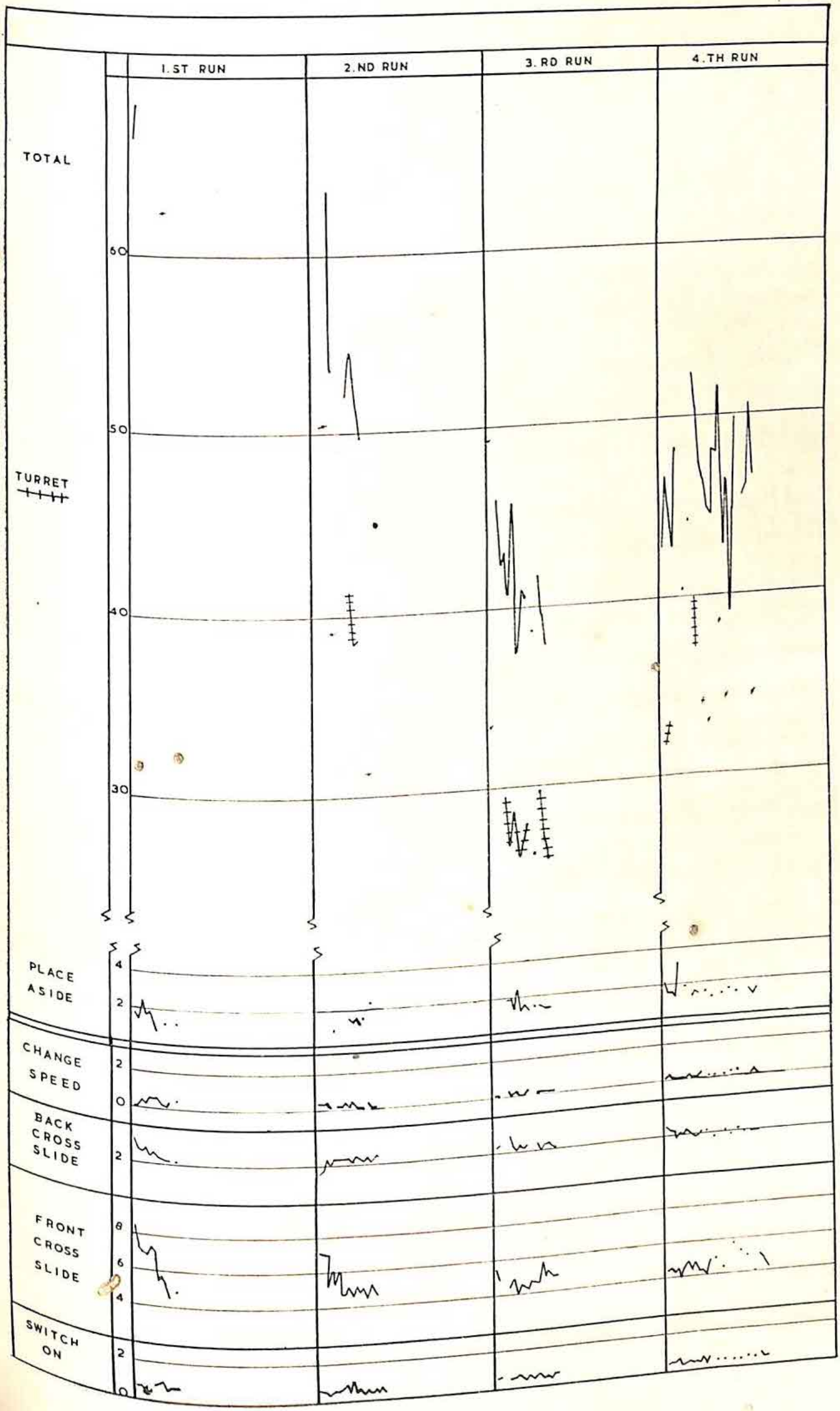
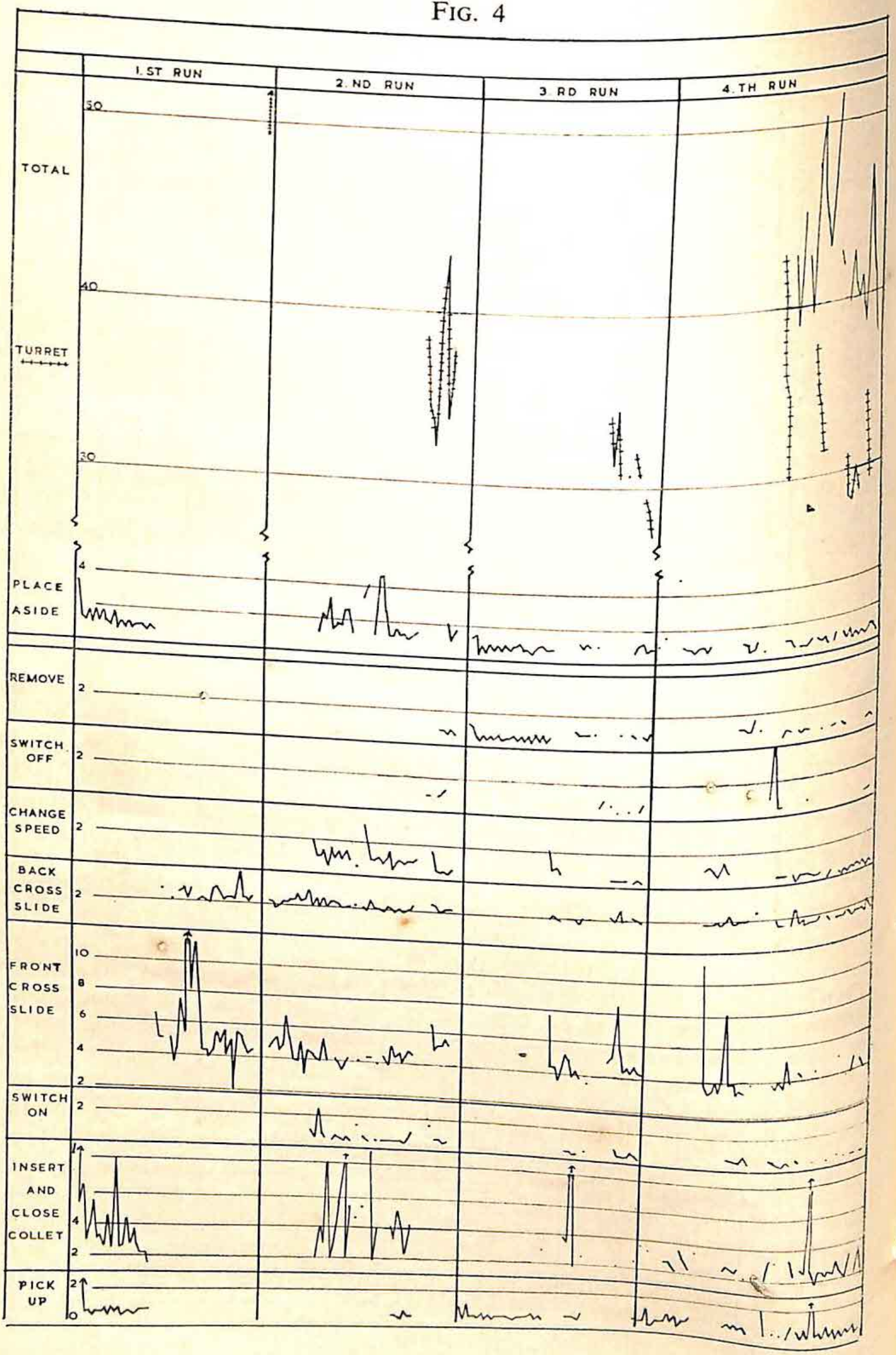

1. ST RUN
2. ND RUN
3. RD RUN
4. TH RUN
TOTAL
TURRET
60
50
40
30
PLACE ASIDE
4
2
CHANGE SPEED
2
0
BACK CROSS SLIDE
2
FRONT CROSS SLIDE
8
6
4
SWITCH ON
2
0

FIG. 4


1. ST RUN
2. ND RUN
3. RD RUN
4. TH RUN
50
TOTAL
40
TURRET
30
4
PLACE ASIDE
REMOVE
2
SWITCH OFF
2
CHANGE SPEED
2
BACK CROSS SLIDE
2
10
FRONT CROSS SLIDE
8
6
4
2
SWITCH ON
2
INSERT AND CLOSE COLLET
4
2
PICK UP
2
0

NOTES ON THE GRAPHS

The graphs are more or less self explanatory. At the top of each graph (Figs. 1, 2, 3, 4) is shown the curve of total time for each cycle of the operation; below the total curve are the curves of individual element times, beginning with the turret, which was always the longest element. The curves of the shorter elements should all appear in one narrow band along the base of the graph, but as their representation in the correct band would be confusing (i.e. the curves would cross and recross each other), the bottom band of the graph has been repeated several times with the scales as shown. Throughout the graphs it is possible to see how each total was made up by glancing down the vertical line on which any point appears; element times for any cycle of the operation appear on the same vertical, except in Fig. 3 where three elements have had to be omitted.

When parts only of the cycle were being practised, the times recorded are shown in the appropriate band of the graph but, of course, without any point on the total curve above them. Gaps in the curves indicate the intervals between one day's practice and the next. Dotted lines indicate that some readings were not recorded. Absence of a recording could occur because the appropriate switch failed to function, or because that for a preceding element failed to work, in which case the only record is a time for two elements together.

DISCUSSION

A survey of the detailed results of this experiment adds appreciably to the knowledge gained from the main results reported in the previous article (Seymour, 1954a). Whereas it was there stated (p.81) that part methods show no advantage over whole methods in acquiring skills in a 'simple' task, it now appears that, although this is the case, there is nevertheless a difference in the way in which the skills are built up by the two methods. It has been shown that a level of skill can be built up on a 'simple' task in a way quite different from that traditionally associated with the normal learning curve which records only changes in total cycle time.

The immediate attainment of target times (i.e. experienced workers' speed) on elements in the first half experiment confirms what has often been observed in industry. On the other hand, the fact that there was not a similar speedy attainment of target time on elements in the second half would support the view that it is the perceptually stringent elements in a task which cause difficulty in learning but which contribute the greatest improvement with practice (Seymour, 1954b). The maintenance, in combination, of a speed of performance which can be more readily attained on elements in isolation did not appear unduly difficult i.e. there is an appreciable transfer of skill from the components of a task to the task as a whole. In view of this, more consideration needs obviously to be given to the arrangements for the combination of elements in the most suitable stages and at the most appropriate time.

Little new information was gained about the transfer of skill from the first to the second half of the experiment, except for the point that the method of learning a task (part or whole) may have as great an effect on performance as the transfer of skill from a previous task.

The more detailed study of the results in the second half of the experiment, in which the part method manifested a superiority indicates that this superiority arises (as far as this experiment is concerned) both from a reduction in the time per correct cycle, and from the fact that less time was occupied in cycles which produced errors.

The greater consistency of results on the part method would appear to arise largely from the fact that more repetitions were possible on elements which needed them, and less time was spent in practising elements which could already be accomplished satisfactorily at target speed.

FURTHER DEVELOPMENTS

The results of this experiment indicate considerable scope for further research, and have shown some of the directions in which it should proceed. In particular, it was obviously desirable to see whether an 'isolation' method of training would give results as good as the progressive part method here used, and similarly superior to those from whole methods, in a 'difficult' task. An experiment on this has already been carried out, and it is hoped that the results will be published very shortly in this journal.

The experiment also indicates the importance of devoting attention to the consideration of perceptual stringency in the elements of industrial tasks, and emphasises the need, in future research, of mobilising our knowledge of perception in experiments on the acquisition of skills.

ACKNOWLEDGEMENTS

The helpful co-operation of the Sub-Dean of the Faculty of Commerce and Social Science in the University of Birmingham, and the sporting assistance of fourteen students from that Faculty are gratefully acknowledged. The work was carried out in the Department of Engineering Production, and sincere thanks are due to Professor T. U. Matthew and his staff for their constant help.

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The Flow of Information

By G. PATRICK MEREDITH

Professor Meredith contributed this paper to a Symposium on Communication held by the Northern Branch of the British Psychological Society at Leeds on 12 February, 1955.

IT is very easy to be carried away by a striking metaphor and to mistake abstractions for physical processes. Information does not flow. It is doubtless correct to say that an electric current flows along a transmission line and from this basic fact of communication-engineering many concepts have been evolved which have conditioned all our thinking about communication. We tend now to think of it as a generalised engineering problem and to forget that electrical transmission is but one highly specialized mode of conveying messages. So highly developed is communication-engineering both in theory and practice that we are strongly tempted to use it as a ready-made frame of reference, and to fit all our ideas about communication into this frame.

An actual study of non-electrical communication at work soon reveals that a set of quite different concepts will be required if a systematic understanding of its problems is to be reached. What happens when a scientific document is placed in the hands of an engineer in a factory? This is the end-point of the " flow " (if that is what we may call the physical conveyance of the document), and it is precisely at this point that most of the difficulties arise. The engineer is not a mere teleprinter or radio-receiver. He is a man with a job, and if the document has any point at all it is going to make some difference to his job if he accepts its message. Probably he has some pride in his job and he may well be satisfied with things as they are. Who is this long-haired scientist anyway trying to teach him his business? It's the practical people in industry itself who have the know-how. These scientific formulae and long-winded descriptions may be all right in a remote university laboratory or research association, but only the people who really work with the tools and materials can possibly know how to improve them.

Psychology, of course, has a word for this mode of thought, but to say that the engineer rationalizes his irrational resistance to change does not solve any of our problems. What is behind the resistance? (I am not saying that there is always resistance but it is the cases of resistance which constitute our problem. If science were widely accepted in industry it would mean that communication had been successful.) Of course I am only taking the engineer as an example—it might be a manager, a director, a company secretary, etc.

I have already begged one question by speaking of an "irrational resistance to change." Granted as a general principle that widespread technical innovation is necessary in an economy which is not quite keeping pace with the demands made upon it, each *particular* change recommended is still a leap in the dark. There is a certain rational self-protectiveness in all of us which urges caution when custom is threatened. And those who are most eager and ready to make changes are not always the soundest members of the community. Many changes entail a whole chain of repercussions, many of them hard to anticipate or to calculate. The decisive factor would seem to be the habitual willingness prevalent in a firm to give objective consideration to any reasonable proposal as it arises, to examine it on its merits and, on occasion, to take a chance with respect to the unknowns. This is a factor which varies from firm to firm and from locality to locality. The explanation of its incidence is to be sought in the history of the individual firm and in the history of the industry as a whole, as well as in current economic circumstances. Thus a psychological approach with a psychiatric slant, looking for Freudian mechanisms to account for resistance, is likely to lead us astray. True, industry like any other walk of life has its sprinkling of psychopathic individuals, but research-workers in communication are not going to advance their problem by concentrating on these, for they have no means of removing them, and it is not their function to give them treatment. It may also be objected that they cannot do much about history or economics either. To this point I shall return presently.

To sum up thus far, the understanding of a scientific document depends not only on its intrinsic clarity but also on the willingness of the reader to understand. This, in turn, depends upon his appreciation of the consequences of understanding, in the shape of an ensuing change of practice. And if there is a resistance to change there will be an unwillingness to understand or even to read. The only electrical analogy I can see here is the tuning of a receiver to the appropriate wavelength, a process which depends on resonance within the receiver.

Now let us turn to the writer of the document. In general he is a research-worker and if he is good at his job he will be absorbed in it. But if he is to be responsible for communicating his findings he needs also to be skilled in the arts of communication. And of all these arts the most important is the art of persuading his readers that what he is saying matters to them. There are many technical components in the communication process, but this psychological component outweighs them all. A detective will decipher a scribble on a charred scrap of paper if he thinks it contains an important clue.

This means that the writer has to think of his subject-matter not solely in terms of its intrinsic scientific content but also in terms of its implications in an industrial context. For only thus can he project himself into the mind of his reader. And only by so projecting himself can he hope to find a mode of expression which will persuade the reader that it matters. Now this is a very tall order. Frankly I do not see how the research-worker, intent on his job, can be expected to achieve this feat of projection.

The conclusion I draw from this is that whilst the research-worker must certainly write a report on his work this report should not, as it stands, constitute the communication to the industrial reader. A go-between is indicated. An act of translation is required.

In what I have said so far I have rather played down the influence of the actual form of expression in the document, not because it is unimportant but because its importance is obvious. Part of our research in communication is very much concerned with the actual techniques of expressing knowledge in documentary form. I have no time here to discuss these techniques—they are far more varied and numerous than many scientific writers seem to recognize. This is an additional reason for demanding a translator, whose business it would be to equip himself with a command of all the necessary techniques. But he needs more than techniques. The mere possession of techniques would give the communication-process a purely technical character. The view which I am trying to establish is that in communication technical factors are subordinate to human factors. Communication is a process between persons.

The man in the laboratory is, in outlook, in training and in mode of life, a very different person from the man in the factory. Their problems are different, their languages are different, their values are different. It might be thought a pity that the two have to get involved with each other at all. Yet each presupposes the other. Modern industry cannot advance without new knowledge, and scientific laboratories depend, for the most part, on the profits of industry for their endowment. But mutual indispensability does not necessarily breed mutual affection—otherwise many music-hall jokes about marriage would lose their sting of truth. Thus it may be that the problem of communication from science to industry has more in common with marriage guidance than with electrical engineering. But whereas the demand for marriage guidance sometimes ends abruptly in divorce there is no such ending possible in the marriage between science and industry—only a weakening of the bond at the price of national impoverishment.

That this bond could be and should be a great deal stronger needs no demonstration by me—it has been declared on all sides and is, indeed, the major premiss of our research. I have tried to show something of the nature of the bond, because we cannot design research on a problem without first forming a clear concept of the problem itself. In suggesting that this is primarily a psychological problem rather than a technical problem I am laying myself open to the obvious taunt that naturally a psychologist will see a job for himself in any problem. In this particular case the facts are otherwise. Six months ago in undertaking this research I envisaged it as a problem in applied semantics. Our aim, as I saw it then, was the improvement of techniques of communication. In so far as personal problems might arise they would be, as I thought, the problems of our own penetration as research-workers into industry. To our gratification and surprise we have been welcomed with cordiality. But although the problem of techniques is still evident the upshot of our first six months of interviews has been the clear indication that, over a wide area, science is failing to make its significance felt in industry. Scientific discoveries are not being sufficiently made known in industry because the psychological needs of the industrial reader are not sufficiently appreciated.

This suggests that to think of the process of scientific communication as akin to firing bullets at a target is, if I may say so, to miss the mark. It is more akin to feeding a fastidious cat. Only by knowing what the cat really likes can the task be accomplished. The problems of communication are more acute at the receiving end than at the transmitting end. It was partly through a hunch that this might be the case that we decided to make a start with the *consumer* of scientific reports before tackling the *producer,* though logic would have dictated the opposite procedure. We anticipated a variety of complaints about scientific jargon, indigestible prose, ponderous style, confusing diagrams, incomprehensible formulae and the like. Armed with such complaints we could go to the scientific writers and ask: what about it?

Well, we certainly find complaints about these things and we hope later to indicate possible remedies. But although it was our intention to concentrate our research on documents (i.e. technical reports) the evidence so far collected indicates that it is essential to think of documents in relation to persons. In fact just as a word derives its meaning from the context of the sentence in which it stands so the document as a whole conveys its message only in so far as it enters into a wider context of communication. I want now to consider this wider context.

For the sake of exposition I shall present a generalized and perhaps idealized context whose parts may not be all equally applicable in every actual instance of scientific communication to industry, in fact a conceptual model. What are the ingredients? To start with we have a piece of scientific information—an account of the finding of some piece of research. In general this will describe certain objects or materials, it will state certain of their properties and processes and will indicate certain laws governing the relations between them. It may also provide some theoretical analysis and historical references. It may also provide certain prescriptions for action. These seven common factors—objects, properties, processes, laws, concepts, history and operational prescriptions will be found in the majority of scientific documents. The crucial question from the point of view of our enquiry is this: can these factors all be adequately represented by means of words? A little reflection (supported by some past experimentation) will suggest that different factors demand different media of communication. Words are suitable for theory and history. Graphs are usually needed to represent laws. Objects and properties may be shown by pictures and diagrams, but usually it is far more effective to see and touch the thing itself. We are still left with processes and operations—essentially dynamic factors, and notoriously difficult to express verbally. Some processes are invisible; others, though visible, demand an inconvenient time-scale. The medium *par excellence* for representing processes of all kinds is the film. The film can likewise be used effectively for showing human operations, but as a rule a direct demonstration by the human operator is by far the most satisfactory method.

All this is to say that the document is not a self-sufficient medium for the communication of scientific knowledge. And when I point out that at its best communication demands a two-way relation, transmission and feedback, between sender and receiver, it will be seen that a human agent must in certain ways participate directly in the process. The concept of adequate communication which I am suggesting here, then, is a kind of six-pronged invasion force, the six prongs being verbal, diagrammatic, pictorial, filmic, material and human. These six share the burden of communication between them.

Such an invasion requires an officer in charge of operations. It also requires a full understanding of the respective functions of the six prongs and of their modes of combination. These can be determined partly by experiment and partly by surveying existing methods in which two or more media are used jointly. In many cases the full six-pronged attack may not be needed, and with existing resources it is, indeed, seldom practicable at present. But if it is worth while doing scientific research it is surely also worth while to secure adequate communication of its findings.

This is not the occasion to discuss these six communicative agencies in detail, but I must indicate briefly what I take to be the role of the human agent. For let us be candid about this: the concept I am putting before you demands the introduction of a new functionary. He is not altogether new: we have Public Relations Officers, Technical Liaison Officers, lecturers and so on. I do not know how precisely their jobs are defined or what training is deemed adequate for them. The Communication Officer needed to command our six-pronged invasion would have a basic background and would spend his time in and out of both laboratories and factories. He would be there to answer questions about documents, to recommend films, to suggest visits to experimental plants, to organize demonstrations. He would take a hand in the writing of documents, the selection of illustrations, the design of diagrams. Above all he would act as the human link between the man in the laboratory and the man in the factory.

Is this utopian? I would remind you that less than twenty years ago the release of atomic energy was widely regarded as utopian. A new technological era demands new means of communication. And as new demands are made upon man's understanding the role of the human interpreter becomes more important than ever before.

Group Discussion in Industrial Rehabilitation: A Pilot Study

By I. S. WILLIAMS

Mr Williams is Psychologist at the Hull Industrial Rehabilitation
Unit of the Ministry of Labour and National Service

THE British Council for Rehabilitation has defined rehabilitation as "the whole range of services from the time of the onset of the individual's disability to the point at which he is restored to normal activity or the nearest possible approach to it". One of the last stages of rehabilitation is often a course at one of the Ministry of Labour's Industrial Rehabilitation Units. In these Units, persons who have suffered from accident or illness, as well as others who have been habitually out of work, practise living again in industrial workshops. Because it was thought that group discussions on personal, social and employment problems might assist in this process of industrial rehabilitation, a pilot investigation was carried out between November 1952 and April 1953 in the Unit at Hull.

THE MAIN EXPECTATIONS

It was expected that the discussions would (a) have therapeutic value, (b) be educative, and (c) allow of assessment which would facilitate vocational guidance. It is not suggested that these three functions are discrete; they are treated separately merely for purposes of elementary analysis. In regard to (a), it was observed that the disabled person, who has usually been living in hospital and at home for months if not years, tends to feel isolated and often socially rejected. It was expected that the group discussions would help him to become socially readjusted, that they would give him more confidence in himself and in others. It was also thought that they might facilitate the release of emotional tension centred around problems of grievance or perhaps of guilt.

In regard to (b), it was not intended to use the discussions as a means of disseminating information either authoritatively, which might have been done better by a lecture or talk, or by indirect techniques, such as films, which demonstrate supposedly sample cases. Rather, it was expected that if individuals described and discussed their own 'real-life' experiences, they would get a better insight into their problems and perhaps be stimulated into changing their attitudes towards their disability and their working capacity, for these were quite often inclined to be self-pitying and apathetic. In regard to (c), one of the duties of the author, as occupational psychologist (Vocational Officer) at the Unit, is to formulate vocational guidance. He expected that the group discussions would prove valuable by enabling him to observe each individual's behaviour in a situation which differed from both the workshop and the interview. It was thought that they might disclose tendencies to form sub-groups, or an inclination for the whole group to regard itself as being part of a wider minority group of disabled persons as was suggested by Hanman (1951).

THE MEMBERS OF THE DISCUSSION GROUPS

The members of the discussion groups were living at home or in lodgings and attended the Unit voluntarily for a course that normally lasted seven or eight weeks; they were in receipt of a small subsistence wage, and time spent in discussion was regarded as part of the normal working day. Ages varied between 16 and 65. Men greatly outnumbered women; usually two or three women attended a discussion, but on seven occasions men only were present. Most men had previously earned a basic wage of between £6 and £7 a week; they were mainly labourers from factory, mill, dock, building or land; a few had been seamen, fishermen, clerks, lorry drivers, etc.; occasionally a member was a skilled craftsman or a self-employed business man. The majority had left an elementary school at 14. The distribution of intelligence scores was skewed slightly below the population average.

THE FOUR TYPES OF GROUP DISCUSSION

Thirty discussions were held in which approximately 150 persons took part. The largest group was 25, the smallest 6. For experimental purposes four types of groups were formed which, because they differ essentially in structure, are described separately below.

1: *Groups for Psychodrama*

The first three groups, each consisting of ten members excluding the leader, were formed in an attempt to estimate the value, to this population, of Moreno's psychodrama technique. The members had, previous to the industrial rehabilitation course, applied unsuccessfully for vacant jobs. They were all known to each other by sight but only seldom by name. The author, assuming the role of leader, first invited each one in turn to introduce himself to the others and then asked everyone to consider how he would set about applying for a job. This was discussed briefly and usually very inadequately for about five minutes, and then two of the least inhibited members were asked to act out an imaginary employee-employer situation. The choice and description of a more or less suitable job lasted some two or three minutes, and then the prospective employee left the room while the 'employer' sat at the desk and the remainder of us moved our chairs back six or seven feet and prepared to listen as an 'invisible' audience. A volunteer secretary opened the door, the candidate re-entered and the drama commenced. At first attempts, the conversation usually dried up after a few conventional phrases such as "Good morning", "Sit down", or "So you've come about a job", and either the candidate or the interviewer, or both, turned despairingly to the leader and asked what to do next. The leader refused to be drawn but insisted that they act out the situation to a finish;

often the interviewer, presumably imitating employers he himself had met, chose the easiest course and said, "Well, leave your name and address and we'll let you know when we want you". Then the leader asked the two participants to reverse their roles so that the interviewer now had to apply for the job he had just given or refused. Afterwards the group reformed into a circle and the question as to whether the applicants would or would not have got the job was discussed, usually for about five minutes. Then the leader asked if two others would like to volunteer and the process was repeated until everyone had taken part.

Only once was a member adamant in his refusal to act. The group often offered encouragement but never attempted to persuade. Performances varied both in length and quality, but tended usually to follow a stock pattern; members frequently showed considerable ego-involvement but seldom much adaptability or originality. However, there were exceptions: a young man who wanted work as a cowman had as his 'employer' a middle-aged woman who had worked for many years as an office cleaner in the city. It was suggested that she should play the part of the farmer's wife and the applicant had called while her husband was away. When the young man knocked (this time there was no secretary) she told him impatiently, " Why can't you come to the back door? . . . Well, what do you want? . . . Can't you see I'm busy? " The audience laughed, the lad stammered and looked confused, but after a minute our charlady softened, talked with him, and finally surrendered him the job with the injunction: " Well, put down your bag and go and see to those cows—if we haven't a cowman I expect they're bustin' ".

It is difficult to estimate the usefulness of this type of psychodrama. Ease and naturalness of behaviour appeared to be inversely proportional to the reality of the situation: the more unreal the situation the easier it was to act. Moreover, if it was difficult for a member to imagine himself to be an employer, it seemed to be still more difficult for his 'candidate' to imagine that he was talking to an employer. It may be that the situation in which the 'employer' found himself was so far removed from his own experience that he could act it on a phantasy level, whereas the member taking the part of the 'candidate' could not easily forget that he really was an unemployed person looking for a job. It was as though the 'candidate' was trying at one and the same time to act at two different levels: he had, as it were, to speak *from* his own more or less real level, *to* a fellow whom he had to imagine at a phantasy level. Putting it the other way round, it was like confronting an actor on the stage with a real villain carrying a real revolver.

Although the author suggests that there was usually considerable ego-involvement, the subjects themselves consciously thought that the drama situations were too remote for immediate self-identification. Spontaneously discussing the 'psychodramas' in the workshops, they complained to their supervisors that they were not sufficiently life-like and that it was unusual for a firm to interview a workman formally and that as often as not he was hired casually by a foreman. This point was discussed at the beginning of the next meeting. Most members felt that getting a job was largely a matter of favouritism or luck. But even this point was seized upon: "You don't expect to win a football pool the first time", someone suggested. The disabled person is often so isolated and unsure of himself that he willingly gives up at the first rebuff. "If it's only luck, then why not keep trying until your luck changes?"

Sometimes the acted situations were generalised and less structured so that they covered quite casual engagements; once or twice they were taken from the subject's own experience. But the leader always insisted that the employer-employee relationship should be discussed. What did the employer want from the employee, and what, in consequence, had he to find out? If there were several applicants for the same job, why did he choose one and not another?

It appeared that the hour and a half spent on each of these discussions was not wasted. It is suggested that the 'candidate' role was valuable because, in so far as it meant ego-involvement it also meant experience and, because it was watched and discussed by the others (almost invariably sympathetically), it also meant a gain in self-confidence. It is further suggested that the 'employer' role was valuable because it sometimes enabled a member to 'identify himself with the aggressor', for most of them had, at some time or another, borne a grudge or grievance against an employer; the shared group experience may have left them less isolated and on the defensive, so that when they met the real-life situation they had some insight into the employer's problem and, although it would probably be unconscious, it would nevertheless affect their attitude and enable them to co-operate more successfully.

2: *Groups for Undirected Discussion*

The meeting was called for 3 o'clock; the author took the chair, joined the circle, and asked: "Now, what would you like to discuss?" The first suggestion was: "Who is going to win the 3.30?" The topic finally chosen was "Trade Unions". The dock workers, whose trade union is particularly sympathetic to disabled persons, quickly formed a sub-group which invited and challenged criticism; in a few minutes the others, as though in self-defence, formed a more heterogeneous but completely antagonistic sub-group. Trade unions were soon forgotten and the discussion developed into an argument as to whether there was any job more skilled or more arduous than a stevedore's. Out of sixteen members there were, at this first leaderless discussion, five dockers; they admitted a fisherman and a seaman to the fringe of their group, but were against everyone else. After some ten minutes they considered they had vanquished mining, transportation and building, and started to quarrel among themselves. This sudden fracture of the main sub-group caused the whole discussion to split into fragments and within thirty seconds a dozen people were vehemently talking, in ones and twos, to each other while the remaining four watched them, quietly bewildered.

Three other leaderless discussions were held, the size of the groups being 14, 17 and 22. The conversations were mainly purposeless and rambling. The tendency to form sub-groups, so noticeable in the first group, was lacking; this may have been due to the fact that, whereas the first had been held at one end of a lecture room where members sat on hard chairs, almost shoulder to shoulder, in a small circle, the others took place in the rest room where everyone sat in easy chairs along the four walls, the man opposite was fifteen feet away with a bare table in between. The only occasion when the author observed a tendency for this collection of individuals to behave as a group was when a complaint was made against the administration of the Unit; the complaint was quite a sensible one and amounted to the suggestion that the pay day be altered. However, when the author went round the room asking each individual to state his own opinion, it was found that the majority was quite content with things as they were, and that two members were against the suggestion. This was of course a deliberate interference with the discussion in order to derange the tendency to form this particular type of group; the author, to keep his role of observer, had obviously to check any attempt to make himself either the scapegoat or the leader of the others.

This type of discussion appeared to have little value for this population. However, the following observations may be of interest: (a) The nonchalant, almost apathetic, attitude which individuals often displayed in these discussions amongst themselves surprised the author who, judging from their behaviour in the interview and workshop, had expected a more positive response: periods of complete silence occurred during each discussion, once it lasted as long as 35 seconds—a time which seemed interminable. (b) The members gained some rather haphazard factual knowledge about each others' conditions of work, usually one vied with another, each trying to make his own case appear more disagreeable and dangerous than the rest's. (c) Occasionally considerable emotion was stirred up, but there was no channelling and little release of tension. The majority were onlookers. (d) Grievances were often expressed, particularly against conditions of work, but with little insight into possible ways of redress. (e) Occasionally it was necessary for the author to intervene and correct mis-statements of fact. (f) At the second meeting, a suggestion that the Budget be discussed was barred by the author as being a subject unlikely to do more than vent political prejudice. He asked them to avoid generalizations and hearsay evidence and suggested that they confine themselves to opinions based on facts from their own experience: "Some of you have lived a long time, you've got things to say which would help the others, particularly the younger ones . . ." This suggestion, though well received, was not particularly fruitful: it seemed to be regarded as a sanction for the recounting, in gruesome detail, of stories of accidents and happenings in hospital. (g) At the end of one rather futile meeting, during which an aggressive neurotic had tried to dominate the discussion, one member said to the author: "Why don't *you* choose the subject and tell us something? Isn't that what you're paid for?"

3: *Groups for Directed Discussion on Problems Emerging*

Twelve discussions were held. Once a week, usually after lunch on Fridays, five, or sometimes six, persons were invited to meet in the author's office. They were selected: each had a personality problem which hindered him from feeling at ease when he was with other people, particularly strangers; when interviewed they had complained of feeling nervous in buses, frightened in the cinema, or uncomfortable in the workshop. One group, for example, consisted of a middle-aged woman who had spent the past twelve years in a mental hospital, a neurotic girl who was too frightened even to interview an employer, a physically and mentally immature youth who was dominated by his parents, an ex-Japanese prisoner of war, a young man who hadn't worked for years and, apparently, did not intend to work again. Usually members attended three or four consecutive discussions; as one dropped out another was asked in.

The author, assuming the leadership, briefly explained the purpose of the discussion and asked: "Has anyone any particular problem he would like to talk about?" On no occasion did any member ever directly mention his own real problem, on the contrary, at first they were distinctly apprehensive lest the author, who knew something of their individual case histories, should approach a tender spot. If there were any newcomers the first topic was usually: Why do we dislike discussing our own problems? The group was then asked: "Are you shy? — What is shyness? . . . Are you afraid? — Afraid of what? . . . What is fear?"

To get the discussion going reasonably quickly, it was found necessary to ask direct questions: "What were you doing last night? What is the most interesting thing that has happened to you this week? When were you last deliberately kind to somebody? What did you do? When were you last unkind to anybody. Why?" And then, having received a few instances: "What do we mean when we blame someone? How do we expect others to behave? What do *you* expect?" The last, asked to each in turn, about a given situation.

At his third or fourth meeting a member was usually asked what he intended to do when he left the Unit. The others were then asked to give their opinions, even though they knew nothing about the job in question: "Does she *look* like a shorthand-typist? Can you imagine her sitting in the General Office?" This last procedure was introduced when it was found that members were almost invariably well-disposed to each other (at least superficially) in this situation, and their affirmative answers appeared to give them considerable satisfaction and, one can suppose, some self-confidence.

The discussions were short: after forty minutes the author summarised the points raised and emphasised the need for a positive approach to their problems of inter-personal relationships. The value of these short meetings was apparently slight, for no one ever volunteered any fundamental social behavioural problem. But when asked verbally, everyone said the discussions helped them and always wanted to attend more. In the selection of individuals it was found better: (a) to select persons with sufficient intelligence to be able to get insight into their own problems, that is to say, persons who were capable of relating the rather trivial points discussed to their deeper emotional needs; (b) to avoid introducing two aggressives to the same meeting; and (c) to select a group with widely differing personalities.

The small group discussions enabled the leader to make indirect criticism which would not have been accepted had it been made directly. The author's status, as occupational psychologist to the Unit, is perhaps best described as that of a specialist adviser, but in his role of Vocational Officer he had to allocate people to various workshops and, as necessary, transfer them from one type of work to another; he was usually called "Sir", and there was a tendency to identify him with management. From the point of view of the majority of members, the author's status (as assumed from what they saw of his role) gave him sanction to criticise their behaviour as workmen but did not give him sanction to criticise their social behaviour unless it adversely affected their fellow workers: for example, he could reprove them for being often late, but not for being always dirty. From the social-readaptive or therapeutic point of view the sanctions differed markedly from those allowed to the Social Worker or Medical Officer. It was found possible, however, to use the group discussion as a catalytic agent: a tentative implication by the author often resulted in explicit suggestion or criticism: "If you were an employer, would you give him the job?" Silence. "If you had to choose between John and Harry, whom would you choose?" "Harry." "Why not John?" And then John, slovenly, unshaven and unkempt, would look at the others anxiously, particularly at Harry. And then it was sometimes necessary after the discussion to hammer the point home in an interview: "It doesn't worry me, John, how you look, but what about the others? What did Miss Somebody sitting next to you think?"

Although it is impossible to prove a direct causal relationship between the use of this technique and subsequent changes of attitude, the author suggests that with certain types of maladjusted individuals this method succeeded, at least for the remaining few weeks of the course, when all others available at the Unit had been tried and had failed.

4: *Group for Directed Discussion on Problems Set*

These discussion groups followed the well-known pattern where a self-appointed leader chooses and introduces the topic. The size of the group varied from fourteen to twenty-five; within this range size did not appear to affect the value of the discussion; there were always a few members who were voluble and always a few who were shy. Members were not individually selected; usually two workshop supervisors were asked to send their people along to the rest room after the tea-break at 3 p.m.; supervisors themselves were invited and sometimes attended. The 3 p.m. meetings finished at 4.20 to give members time to change their overalls, etc., before they went home at 4.30; but even so, fresh angles were often discussed at work the next day and, occasionally, topics and arguments that had been used were mentioned weeks later during interviews. On the two occasions when meetings were held earlier in the day there was a marked tendency for members to carry on with the discussion in the workshops and for these afterthoughts to develop into quite heated arguments.

Among the subjects chosen for discussion were, *Why do men work?* which was introduced by a discussion of various definitions of 'work', *What is a disabled person?* introduced by a short summary of the Disabled Persons (Employment) Act of 1944, *What are natural abilities?* introduced by a discussion of a film, shown previously, on vocational guidance and issued by the Youth Employment Service, *How to get a job,* introduced by asking each member in turn how he came to get the best job he ever had. It is suggested that the topic chosen should be one of which all should have some personal knowledge as well as interest; after a discussion of 'natural ability' one member suggested that it would have been more interesting to discuss a film *Walkabout* which portrays the customs of the Australian aborigines—he was immediately contradicted by another who said: "But I don't know anything about Australia . . ."

In an attempt to estimate the popularity of this type of group discussion the following interest scales were handed out to members by their workshop supervisors on the days following two of the discussions. The supervisors, whose own attitude to the discussions appeared to be one of tolerant scepticism, were given specific instructions that the forms were to be filled in anonymously.

In each statement cross out two of the three items A, B, C.
Write any comment you have below.

(1) I find group discussions	A interesting B pass the time C boring
(2) I find group discussions	A don't affect me B help me C upset me
(3) I would like a discussion	A never B sometimes C once a week

The numbers of affirmative answers to each item are tabulated below: One incorrectly marked form has been excluded.

ITEM	FIRST DISCUSSION	SECOND DISCUSSION	TOTAL
(1) A	12	16	28
B	2	2	4
C	1	0	1
(2) A	2	1	3
B	13	17	30
C	0	0	0
(3) A	2	0	2
B	1	2	3
C	12	16	28

It is obvious that a large majority of these people found these discussions interesting and helpful, and that they would have liked to attend one at least once a week. Not everyone made comments, but some that were made are illuminating: "I think they are the means of finding work" and "Any discussion, on any given thing, is a help to anyone", and "The reason I think discussions are interesting is because you hear other people's views. Also, the reason they help me is because they give me a different outlook on life. The reason why I should like them every week is so that we can keep up to date". One member wanted "Discussions on more understandable items than such as what is work which is hard for the majority". Another commented: "I think it would be a good idea to have another group on the same discussion as us, then pick out the best of the two groups and get their views. It would give you a better idea, and also *us*". The difficulty of devising an intelligible scale is illustrated by two other answers: one member who found that discussions passed the time, did not affect him, and wanted them sometimes, wrote: "Discussions are helpful, but a suitable occupation would be better"—he had struck through item 2.B *I find Group Discussions help me*. Another, who found discussions interesting, helpful, and wanted them once a week, wrote: "Discussions are very interesting but they don't help the individual".

At another discussion an unsuccessful attempt was made to measure immediate attitude changes in order to investigate the possibility of reaching group decisions, for, as Lewin (1947) reported, "certain methods of group decision prove to be superior to lecturing and individual treatment as means of changing social conduct".

It is possible that, in a discussion in which most members necessarily become ego-involved, it is better at some stage to present a hypothetical case in order to arrive at a group decision. The group could, for example, reach a decision as to whether a hypothetical Mr X should or should not be advised to apply for registration as a disabled person. Members could then be asked whether they were individually prepared to accept the advice which they, as a group, had given. It was noticed how vehemently two members insisted upon recounting their own 'hard-luck' stories. They were too full of grievance and (or) self-pity to consider the problem with much objectivity. The discussion did not change their minds, though neither did their lamentations influence the others. If the very fact that they were more than ordinarily ego-involved prevented them from identification with the group, then discussion of an abstract case, *i.e.*, the lifting of the discussion to a level of unreality might enable them to 'unfreeze', to use Lewin's expression. But the unfreezing would first take place at the unreal level and it would still be necessary to identify this with reality; for it is not every member who has sufficient insight into either his own problems or the purpose of the discussion to be able immediately to identify himself with the hypothetical Mr X.

The group situation may, however, in some cases provide the leader with sanctions which will short-cut the gaining of insight. Remarks such as: "Do you think it is right for a man to exaggerate his disability and go on drawing Industrial Injuries Benefit when he knows that he is quite capable of going back to work? " are accepted in a group situation, whereas they might be resented in an interview. Usually the more forthright and honest members answer straight away and quote instances from their personal experience of cases of deliberate malingering. If there is a malingerer in the group he will be faced with the alternatives of defending malingering or of appearing to agree with the group. If he decides upon the first he has almost inevitably to quote his own case, for otherwise he is unlikely to impress an audience which is too familiar with real instances to be satisfied with abstract argument. If he describes his own case he has necessarily identified himself with Mr X and the ensuing discussion will, if it follows the usual pattern, mean good-natured criticism, questioning and suggested explanations from other members.

EVALUATION

Before attempting to evaluate these four types of discussion it is necessary to consider in what sense they can be termed *group* discussions. The larger groups (types 1, 2 and 4) existed usually only for an hour and twenty minutes, then the group split up and never re-formed. The smaller (type 3) groups existed intact for only forty-five minutes; when it re-formed, the following week, it had usually lost one of its five or six members and had to assimilate another. But although the groups were evanescent they were more than mere aggregates: the members were known to each other, if only by sight; with one or two exceptions the members had all lived for several years in the same city and usually assumed, though sometimes quite erroneously, that they all had similar culture patterns; by coming to the unit they had expressed, ostensibly at least, that they had a common purpose—to get back to work; the groups themselves, even the leaderless groups, had an aim—to discuss something. It was found in practice that although the word 'group' cannot be used with the meanings it had for Le Bon, McDougall or Trotter, to give examples, the behaviour of the individual in the discussion often differed markedly from his behaviour in either the interview or the workshop: emotionally it was exaggerated, at least overtly; if he had been shy during the interview he tended to be even more shy in the group, if he had been talkative in the workshop he tried to hold the floor during the discussion. It was as though the proximity of other persons and, sometimes, the impact of their suggestions, became a challenge to the individual's ego and he protected himself with his habitual 'defence mechanisms'; the workshop situation being less personal was less dangerous; the interview situation, though more personal, was sympathetic and the subject matter confidential, moreover, the person being interviewed was forewarned and in consequence more or less forearmed; the group situation, on the other hand, was relatively strange and therefore, one can suppose, relatively more fearful.

If it be allowed that these discussions were in fact *group* discussions and not merely arguments amongst aggregates, it is relevant to mention the role of the leader. There were times during almost every one of these discussions when the leader found himself in an extremely tenuous position. As an occupational psychologist he had neither the time nor the training to attempt any formal psychotherapy: on the contrary, he had sometimes deliberately to break an attempt at identification so as to avoid a possible transference situation. Yet, during his interviews, when he had necessarily to attempt to assess permanence of interests, strength of motivation, etc., he presumably sometimes became associated with an individual's childhood memories and emotional needs. Therefore he had not only to steer the discussion away from the Scylla of heated and misinformed controversy which might easily have devoured it, but at the same time to keep clear of the Charybdis of the Oedipus situation which may possibly have engulfed some members particularly of the smaller groups.

Freud (1921) said of groups which have a leader but have not secondarily acquired the characteristics of an individual: "A primary group of this kind is a number of individuals who have substituted one and the same object for their ego ideal and have consequently identified themselves with one another in their ego". It is unlikely, in any of these discussions, that members substituted the leader for their ego ideal, but it may have been that some members, individually identifying themselves with other members of the group, created a new, nebulous and transient group ideal. By group ideal is not meant a 'group mind' but the modification by the individual of his own ego ideal according to his idea (conscious or unconscious) of what would be acceptable to other members of the group, and the projection of this modified ego ideal upon other members of the group or upon the group as a whole. For Freud (1914) also wrote: "The ego-ideal is of great importance for the understanding of group psychology. Besides its individual side, this ideal has a social side; it is also the common ideal of a family, a class or a nation". And again (Freud, 1923): "Social feelings rest on the foundation of identifications with others, on a basis of an ego-ideal in common with them".

Whatever the explanation, the overt behaviour of these groups appeared to be determined, not by the lowest common multiple of their instinctual needs, but by the highest factor common to their various ideals. In their conscious expression they were usually tolerant, unselfish and helpful. In this respect it is suggested that the finding of Maxwell Jones (1952) and others "that society as represented by the unit community can itself act as a therapeutic agent" can be extended even to elementary experimental groups such as these.

In vocational guidance, a psychologist is sometimes faced with a situation where he knows that any direct suggestion or advice is likely to be disregarded because of his client's anxiety or environmental stress. In such cases it might be expected that he would use permissive techniques of the Carl Rogers kind, but at an Industrial Rehabilitation Unit where he has at one time between eighty and a hundred 'clients', most of whom he will see for only seven or eight weeks, he will only exceptionally have sufficient time to deal with such cases adequately. Rogers (1942) has said: "If the initial stages of counselling are badly handled, the contact is likely to be broken and the client can then rationalize his conflict along these lines. He can say, 'I wanted help, but all that the counselor did was to give me impossible advice. It proves that I have made every effort, but that I cannot be helped'." It is difficult to see how, by individual treatment, one can avoid "bad handling", for, according to Rogers, any direct suggestion during the early stages is bad handling. However, as new techniques of group discussion are evolved, it may be possible to lower the threshold of a person's resistance to suggestion (consider mob behaviour, for example) and, by using indirect group methods, to effect new insight and re-orientation, even where it is impossible—as so often happens in cases of serious injury—directly to alleviate anxiety or physical stress.

It is evident that much of the present therapeutic value of rehabilitation units is a result of social readjustment in workshop, canteen and gymnasium. It is suggested that group discussions, properly handled, can be a means whereby the process of gaining insight is short circuited. Knowledge of other people's difficulties and the ways in which they are being surmounted, their hope, courage, sympathy, friendliness and cheerfulness is not left to a chance remark in the dinner queue or under the gymnasium showers, but is brought face to face, a direct challenge. It may be that the interview is necessary for fact-finding, the workshop for reality-testing and habit-formation, and that the group discussion can link the two.

CONCLUSIONS

Results are summarized in the following table. It appears that Type 1 and 3 discussions require fuller investigation, that Type 2 discussions were of little value, and that Type 4 discussions are most likely to be of immediate use to an occupational psychologist.

Regarding the suggestion that these discussions might disclose tendencies to form sub-groups, there was little evidence to support this except in the B groups. Regarding Hanman's (1951) contention that "branding the disabled actually creates a minority problem", although there may be a tendency for society in general, and some employers in particular, to regard disabled persons as 'outcasts', the persons taking part in these discussions seldom regarded themselves as belonging to a minority group; rather, there was occasionally a tendency for certain members to behave as though they were living in a social 'no man's land'. Compare with this Lewin's (1952) suggestion that "The marginal man shows a typical aversion to the less privileged members of his own group. This can be noted in the hostile attitude of . . . the hard-of-hearing against the deaf".

From such superficial, spasmodic and subjective observations it is obviously impossible to draw specific, objective conclusions. The study did, however, strongly suggest that in the environment and with the population described, group discussions can have therapeutic, educative and diagnostic value; and that the form and function of group discussions in industrial rehabilitation are worth further investigation. Some tentative conclusions are given in the table appended.

TABLE: SOME TENTATIVE CONCLUSIONS ON THE VALUE FOR CERTAIN PURPOSES OF FOUR TYPES OF GROUP DISCUSSION

	TYPE 1 GROUPS FOR PSYCHODRAMA	TYPE 2 GROUPS FOR UNDIRECTED DISCUSSION	TYPE 3 GROUPS FOR DIRECTED DISCUSSION ON PROBLEMS EMERGING	TYPE 4 GROUPS FOR DIRECTED DISCUSSION ON PROBLEMS SET
Therapeutic value	Possibly high for a few cases. Value of audience participation probably slight.	Low	Possibly high	Possibly high. (Over 90% claimed that it 'helped.')
Educative value	High in certain cases if carefully structured	Negligible	Could be high	Considerable
Value for assessment	Possibly high predictive value for the help the member will need when he tries to find work	Could be useful for diagnosis of social maladjustment	Low by comparison with other types	Useful
Difficulties encountered	Time consuming	Numerous	Time consuming, though less so than type 1	Probably the most exacting as a task for the leader

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Afterthoughts on Fawley

By AILSA P. GRAY

This paper was read to the Industrial Section of the British Psychological Society on 14 July, 1954.

THE report on the construction of Esso Refinery, Fawley, received considerable publicity when it was published in March, 1954, and probably needs no elaborate introduction. In this paper I should like to do two things: add something to what has already been said on the methods of the survey, and single out one of its conclusions as particularly worth further thought and study.

The Fawley refinery was officially opened by the Prime Minister, Mr C. R. Attlee, in September, 1951. It was hailed in the press at the time as a very considerable achievement for a number of reasons: it was a very large construction job, which cost £37,500,000 and was the largest oil refinery ever built at one time anywhere in the world; it was completed in 27 months, and the major units were in operation some months before the date that had been originally scheduled; and it was built by a combination of British labour and American management. It appeared to provide, right at home and in a dramatic way, an object lesson in American methods of achieving high productivity. For these reasons, the Productivity and Technical Assistance Division of the Mutual Security Administration Mission to the United Kingdom commissioned Research Services Limited to find out how it had been done.

By comparison with some other studies of management made recently, ours may appear slight in method and superficial in content. It is a broad general survey, with no very deep probing into particular aspects of organisation or sets of relationships. It took this form in part because we were called in when the work had already been virtually completed, and so had little opportunity for direct observation of the day-to-day working out of procedures, in part because the time available for research was limited. A draft report was completed five months after field work started, and the field work itself occupied not more than about eight man-weeks. Within this scope there was little opportunity for elaborate testing of techniques, or for exploring the many subsidiary problems which some of the American practices suggested to observers.

These limitations of resources were to some extent offset by very good fortune in other ways. The first of these was the public discussion which attended the official opening of the refinery. When we started work, there was a great deal more about Fawley already in print than those setting out to study an industrial undertaking can normally hope to find. In particular, the *Petroleum Times* had brought out a special issue devoted to Fawley which contained, in addition to much technical detail, several long articles on the planning and construction of the refinery. These indicated in outline many of the points which we later studied in greater detail.

The second piece of good fortune was the generous co-operation given by the various firms concerned—the owners of the refinery, Esso Petroleum Company Limited, the Standard Oil Development Company, which represented the owners on the site, and the main contractor, Foster Wheeler Limited. We have acknowledged in the report our debt to members of these firms for giving up a large amount of their time to us, and for allowing us ready access to their files and records. What we did not mention there was that S.O.D. also gave us the almost full-time assistance of one of their own staff, Mr. Norman Hinxman, a member of the permanent refinery staff who had been lent to S.O.D. as Personnel Officer during most of the construction period. Mr Hinxman spent endless hours putting us in the picture from the start, answering our at first very vague questions, explaining who was who, arranging introductions and appointments and getting hold of files.

Between them, Mr Hinxman and the *Petroleum Times* greatly reduced the inevitable preliminary period of fumbling around, getting one's bearings and building up sufficient background information to begin to see what questions needed asking and what problems needed investigating. Our introduction to the job was completed when the Project Manager, Mr R. Cole, introduced us to the team of American engineers and supervisors at one of the regular Friday morning progress meetings.

Thereafter we relied on three main research methods. Direct observation was, I have mentioned, of limited use, but it helped to provide some background colour for the report, and gave us impressions of certain details, such as the tone of relationships between supervisors and workmen, with its atmosphere of informality and good humour. The remaining two sources were the files and records of Foster Wheeler and S.O.D., and personal interviews.

Most of the interviewing was informal, and no set questionnaire was used. In the case of the middle management strata (the U.S. supervisors and engineers and the British office staffs) we had no need to feel that we had to get all the information we needed at one go. We usually talked over the two or three problems which were occupying us at the moment, with the knowledge that when new problems cropped up we could go back. The one exception to this was the top men, for example Mr Cole, and the Co-ordinator of Refinery Production for Esso, Mr Jenkin. They were very busy men, and we had to prepare the ground carefully before we saw them to make sure that nothing important was left out. For this reason we left such interviews until fairly late in the field work period.

Interviewing on the management side did not set any serious sampling problems. There were a number of people whose official positions made them key informants—the Materials Controller, the Labour Manager, etc. The group as a whole was fairly small. There had been 70 supervisors, but some had by this time completed their work and returned to the United States. We talked with as many of the remainder as we could get hold of. When we came to consider how far we would be able to study the reactions of the British workmen, however, we were faced with a very considerable sampling problem. When we were at Fawley, the labour force was very much smaller than it had been at its peak. A representative sample would have had to include men who had been paid off as redundant, men who had been dismissed for various other reasons and those who had left of their own accord, as well as those still working on the site. Organising the selection and interviewing of a systematic sample even of men still working at Fawley would have required considerable resources, and unless supplemented by samples of those who had left, which would have made much greater demands on time and money, would have been of doubtful utility.

In the event, we did not carry out any systematic sampling. The opinions quoted here and there in the report are those of informants who were still employed at Fawley during our field work period. They were taken on the site, and we tried to cover all trades and all levels of skill and authority. Beyond that, the principle of selection was largely that we picked out men who at the moment looked as though they could be interrupted without creating confusion in the work. Again, no set questionnaire was used. We usually started by asking what trade our informant followed, how long he had been at Fawley and what his previous job had been. Then we asked how Fawley compared with his previous job, and this usually started a discussion quite as long as one could reasonably ask, standing out of doors in a cold December wind.

Opinions expressed in these interviews have been quoted in the report for whatever interest they may have. Since the interviews were largely non-directive, it may be assumed that the aspects of Fawley organisation which were covered were those which seemed most important to our informants. But the quotations do not indicate the views of a representative sample of the work force, and they are probably the least reliable part of the report. In addition to these interviews taken on the site, we had much more lengthy discussion with a number of shop stewards taken in the privacy and relative comfort of an office.

In building up from these various sources a picture of how the construction of the refinery was organised, the problems were mostly of a quite familiar kind—cross-checking of information received from different individuals and different sources, etc. A number of myths about the refinery had sprung up (more have appeared since the publication of our report) but many of them were easily disposed of. There was, for example, the gentleman living a few miles from Fawley who wrote to one of the daily papers to explain how 'honest hard work for honest pay' was obtained: "If a man slacks he is spotted by an overseer, who reports to the office by a microphone he carries. The case is watched. More slacking and the man is called to the office and sacked. If he protests he is shown a compromising photograph of himself, against which there is no argument. He loses his job. The men like it this way . . ."

But the main problem was that informants often described organisational procedures in a sort of technical shorthand that at first conveyed practically nothing to us. We usually had to ask a long series of follow-up questions before the procedures used became understandable. Scott and Lynton have described, in *Three Studies in Management,* experiences rather of this kind which they had in studying a Scottish colliery. The typical example they give is of an explanation offered for a piece of individual behaviour or a trend within the colliery which is in fact a highly conventionalised selection and simplification of the factors at work. This was not the typical form of the difficulty in our case. What we had to do more often was to expand and make more concrete a very abstract and generalised statement of a particular policy or procedure. The Americans at Fawley possibly used more set phrases of this kind than usual because their organisational policies had been very consciously and explicitly worked out. We were constantly told things like these: "Control on the job was control of achievement, not of cost," "Vigorous expediting was most important," "Materials were ordered up to two years in advance of when they would be needed." These phrases do not immediately give an understandable picture of concrete human activity. We had to ask follow-up questions until we got a reasonably clear mental picture of a particular group of men engaged in a particular activity of a generally familiar kind.

One rather minor example will be given. The reports and charts showing the progress of construction all gave for each unit a figure showing the 'percentage complete' by a particular date. We had seen some of the units referred to. There was, for example, the catcracker, a towering group of steel cylinders encased in a complicated scaffolding and festooned with wreaths of pipes. We knew that its construction had involved many kinds of work from digging trenches and pouring concrete to adjusting complicated electrical gauges. We might well wonder how one estimated what percentage of that mysterious structure had been completed. So we asked how the figure for 'percentage completed' was reached. The first reply was simply that 'it was a physical estimate'—which did not seem to make anything clearer. So we asked how the estimate was arrived at, and were told 'by measurement in many cases.' It was still impossible to imagine a group of men measuring up the catcracker, and we asked what was done where direct physical measurement was impracticable. The answer was that the estimate of the inspecting engineers was used, and that to make sure that it was reasonably accurate it was checked by both the contractor's and customer's engineers. This seemed at last understandable. We knew that a common unit applicable to all types of construction had been provided in advance by the estimated manhour budget, which was worked out for a very detailed breakdown into construction stages for each unit, and we could picture the type of activity involved.

In many cases, this process of filling out a brief phrase or a brief description was spread over a large number of interviews. This was the case with references to the early ordering of supplies. It needed persistent probing about who ordered what and how they knew what to order before the details of the complex supply system with its multiple safeguards against failure became clear. Part of this information was collected from many different people, with intervals between discussions for sorting through our notes and trying to work out the outlines of a description of the system, before going back to fill in the gaps.

The brief we had been given for this survey had not been detailed and specific, but very general: we had been asked to study the methods by which the construction of the Fawley refinery had been completed ahead of time. In our first explorations and discussions of the subject we had concentrated on labour-management relations as the presumed key to success. We had regarded our task primarily as that of making a study of relations between American management and British labour. There were doubtless various reasons for this. One feature of Fawley's history was that there had been no major labour disputes during construction, and that only a small fraction of one percent of the manhours expended had been lost through labour disputes. Then there had been various unusual features about the union contract, which gave rise to the question of how they had come to be accepted. But this initial focus on labour-management relations probably also reflects the balance of emphasis in current industrial research and discussion generally.

However as the research progressed, we had our attention drawn in various ways to a variety of purely intra-management policies and systems which seemed very relevant to our brief. In the final report the description of these took up very much more space than discussion of the reactions of British workmen to American practices, and we barely touched on the question of how innovations came to be accepted. This focus on management is, I think, one major reason why the Fawley report has aroused so much interest. (The other major reason is perhaps that it had in Bob Cole an undoubted hero, and one with considerable gifts for vivid self-expression.) Contrasts with British management, which were made sparingly in the report, were readily supplied by commentators, and the inference was drawn that if British productivity were to be raised, the need for new systems and attitudes among management was perhaps even greater than the often reiterated need for harder and more conscientious work from the lower ranks of employees. (This is not, of course, a novel conclusion. It is, for example, discussed at length by Graham Hutton (1952) in *We Too Can Prosper*. But it has received much less attention than have the problems of morale and productivity among employees.)

I would like to single out from among our conclusions about management at Fawley not a specific organisational method like the use of one main contractor, or the continuous close contact between contractor and customer, or the attention given to making sure that supplies arrived when they were needed, but a fundamental attitude of mind, the attitude which produced the really formidable planning and system of organisation at Fawley. Our report includes a section on differences in attitude between United States and British management. We included it rather reluctantly, because it seemed a rather intangible amorphous factor, difficult to describe precisely and impossible to measure with the resources at our disposal. But we were driven to conclude that the success in operation of the various policies which we have described depended on a general, underlying, typically American attitude.

We have quoted in the report a statement by an anonymous British engineer who described the difference in attitudes more forthrightly than I would probably have dared to. He wrote: "Most important of the reasons for higher productivity in the U.S.A. is the general atmosphere there that nothing is impossible and most things are worth trying. In contrast, the attitude here seems to me to be that most things are impossible and not worth trying." This seems to me a good, if somewhat extreme, description, except that I would add to the second sentence the qualification, "except those that have been tried in the past."

This is, of course, a very general statement. But such an attitude was revealed in many ways at Fawley. Perhaps the most important and striking of these were, firstly, the belief in advance planning. Very detailed planning was one of the things that the Americans believed to be not merely remotely desirable, but possible and necessary; and if you ask: "How did they do it?", the first step in answering is to say that they made up their minds that it damned well had to be done. Moreover, they planned in advance not merely what was to be done, and how it was to be done, but what difficulties were likely to be met and how they would be overcome. For example, materials were ordered early, but they were not content with merely receiving a promised delivery date. There was continual and systematic checking up with suppliers to make sure that the promised dates still held good. A second aspect was the belief that plans once made could and should be adhered to. Thirdly, the Americans brought considerable determination and a great deal of hard work to the task of overcoming difficulties, foreseen and unforeseen. They were not always ready to seize on excuses for failing to achieve their target.

I certainly do not wish to claim that all American firms are permeated by this outlook, and that no British firms are. But it does appear to be true that it is more common in the United States than in Britain. Something of the kind has, for example, been stressed in nearly all the reports of the Productivity Teams which went to the United States under the scheme sponsored by the Anglo-American Council on Productivity, and by some of the United States teams which visited Britain. I happened to have two of these reports, on subjects in which I am interested for reasons quite unconnected with Fawley, at hand while writing this paper. I looked them up to see what they had to say. The first was the report of the British team on retailing, which writes: "The American often seems to be much less evolutionary and more empirical than we are in this country, more ready to apply his theoretical solutions in practice, and if necessary to scrap old equipment, or—what is more difficult—old ideas." The other report was that of the United States team which studied the British cotton industry.

Its report comments on these basic attitudes several times, and the following is only one of several similar statements: "Complacency, reluctance to depart from things of the past, and lack of incentives are the greatest barriers to productivity and the greatest dangers to the survival of the British cotton industry." Other reports of research carried out in Britain have pointed to the importance of basic attitudes towards change, but, so far as I know, the question of how such attitudes are determined is virtually untouched. Fawley, in this respect, is probably not typical of America. It has been stressed, in our report and in the press, that for suppliers and workers alike the size and importance of the refinery helped to arouse enthusiasm for the job. What has not been stressed is that the same thing was true for the Americans at Fawley. For them also it was an unusually large and important job, and pride in sheer size has often been regarded as an American characteristic. Moreover the construction schedule was regarded as a tight one. The American managers do appear to have regarded Fawley as a challenge. Then, too, they were working in a foreign country, and may well have regarded it as an opportunity to show what Americans could do. Attitudes of national pride may have been involved. Finally, it was a job with a definite time limit. Setting up a system and working hard to keep it going did not involve the prospect of a monotonous grind stretching endlessly into the future.

But although this 'American outlook' may have been intensified at Fawley, there is, I have suggested, plenty of evidence to suggest that it exists elsewhere. It is easy to think of some characteristics of American society which may help to produce it. In the United States, compared with Britain, a greater degree of respect and social recognition appears to be given to success in management as a career. Secondly, there is more social mobility, not merely up and down the social scale, but from job to job and from firm to firm. Then there appears to be a lesser degree of class consciousness. America is not the classless society, but its class structure is very different from that of Britain, one of the differences being that there are fewer obvious symbols of class status in such things as accent, manners, dress, eating habits, etc. It is conceivable that this difference may affect attitudes within industry in a number of ways. It may be, for example, that Americans feel less need to stress their class position by fostering and preserving the marks of their status even when this concern conflicts with the demands of the work situation.

The comparison with the United States should prove fruitful in suggesting some of the factors at work, of which these are only a few of the more obvious. But the important problem is not to explain differences in the industrial climate between Britain and the United States for its own sake, but to try to understand the sociological and psychological factors which produce such differences in outlook wherever they may be found. And, in spite of all that has been said, Britain can certainly provide examples for study.

This whole subject seems to be one of the most important that social scientists could undertake at present. The first need is probably for a more precise and comprehensive analysis of types of attitude, and a search for indices by which they may be identified. Fortunately, the number of psychologists and sociologists who are in a position to contribute to the study of these problems is increasing. If the Fawley study has helped to draw attention to them, it will have been well worth doing, whatever limitations it may have as a piece of scientific research.

Book Reviews

The Reach of the Mind. By J. B. RHINE. London: Penguin Books, 1954. Pp. 198. Price 2*s*. 6*d*.

This is Professor Rhine's third book, first published in 1948, and now reprinted and published as a Pelican Book. In it he continues the account of the investigations into psi-phenomena which have been carried out in his department at Duke University since 1930, the early stages of which were reported in his book *Extra-Sensory Perception*. In the main he is giving in popular form a straight description of the many experiments which he and his colleagues have made and of the statistical methods used to evaluate their results.

This is not the place for an essay on the findings of para-psychologists and the relation of their subject matter to psychology as it is ordinarily presented. It is, however, worth considering why so many people have found demonstrations of the existence of telepathic, clairvoyant and psycho-kinetic powers difficult or impossible to accept. Is it because they suspect the experimental techniques? Perhaps, but it is difficult to see at what point these can be faulted; certainly the methods employed by the Duke University investigators in their later work leave no ground for criticism in this respect. Do they doubt the statistical basis of the argument? Probably a good many do, but Professor Rhine has an answer to them in the statement made by the American Institute of Mathematical Statistics in 1937: "Assuming that the experiments have been properly performed, the statistical analysis is essentially valid. If the Rhine investigation is to be fairly attacked, it must be on other than mathematical grounds".

What other reasons can there be for rejecting the evidence in support of the existence of psi-phenomena? One is no doubt attitudinal; these rather curious experimental results are difficult to fit in to our framework of knowledge and in many cases may conflict with our existing philosophies of the nature of things, and are consequently ignored. Another is, I suspect, lack of first-hand acquaintance with the vast amount of careful experimentation that has been done in this field, not only in the U.S. It is perhaps unfortunate for the para-psychologists that their subject in this country still goes by the name of Psychical Research, a title chosen in 1882 when the Society was founded, but now having a connotation of ghost-hunting, fairies and witchcraft for many people. Perhaps it is not realised that investigations into psi-phenomena can be carried out by scientific method by serious scientists, and the results published like the results of any psychological experiment.

Those interested in this field of study will very likely have read this book already; others will certainly find themselves stimulated, although maybe in at least partial disagreement with some of Professor Rhine's ideas. You cannot expect more for a guinea, let alone half-a-crown.

D. C. S. WILLIAMS

The Dock Worker. An Analysis of Conditions of Employment and Industrial Relations in the Port of Manchester. By THE UNIVERSITY OF LIVERPOOL DEPARTMENT OF SOCIAL SCIENCE. University of Liverpool Press, 1954. Pp. 277. Price 17*s*. 6*d*.

Field research of the kind reported here is much needed as a check for theory and a guide for practice in the industrial domain of the social sciences. A good feature of the research is careful and responsible appreciation of the difficulties of surveying attitudes in such complex, changing, and turbulent conditions. The sifting of historical evidence and authoritative opinions, thorough reconnaissance on Merseyside, careful entry at Manchester, strict sampling and re-checking, exploitation of informal occasions to check and complement results, adroit use of objective evidence for checking and inference—such procedures as these make the research a useful illustration of method. Also, the investigators were alert to gain as much from their method and their position as possible: they made good use of the rare opportunity to study a strike closely.

To the psychologist the strong underlying feelings of dockers will probably appear the most important source of problems. Hostility shows out against the employing company, the Dock Labour Board, the Trade Union, even against dockers from other ports. This continues past tradition except for the significant transposition of the Trade Union, once the vanguard of militant action, into the ranks of the enemy. Basic causes lie in a history of economic privation and insecurity, bad working and living conditions, and social inferiority. Discontents of the present readily attach to these strong feelings and habits from the past. This is not peculiar to Manchester, or even to the United Kingdom. Strike comparisons by Kerr and Siegel (see *Industrial Conflict*. Edited by A. Kornhauser *et al*. New York: McGraw-Hill, 1954) show a universal record of extreme unrest among miners and dockers, which is attributed mainly to the social isolation of such groups. Although this factor shows up clearly in the Manchester study, it may deserve more emphasis. On the other hand, further comparative study of industries and nations would be necessary before confirming the importance given to 'decasualisation'.

Administrative and legal complexities and long-term national union policies do not mean much to these dockers; and consequently well-meant reforms often merely cause confusion and attract the pervading hostility. The investigators note similar reactions in the United States, and the reviewer can think of examples from Australia, where 'legalism' flourishes. The investigators see a remedy in better communication, but point out that there must first be a clear and simple picture to communicate. Also the ruling feelings of hostility make communication more difficult. The reviewer would suggest 'project consultation', *i.e.*, participation by dockers in committees to tackle specific jobs of obvious common importance, such as safety, sanitation, perhaps a Port Newssheet, as a worthwhile approach. The research showed unofficial leaders as prominent in the strikes; and such consultation may give alternative scope to potential leaders. A little time given to such matters on the call stand could provide a co-operative antidote to the painful competitive feelings engendered there.

This only touches the material presented in this book. The changing position of the Union, which reached a later stage in the 1954 nation-wide dock strikes, particularly repays study.

The investigators and their sponsors are to be congratulated on the provision of valuable knowledge. One is almost ashamed to mention imperfect indexing and involved prose.

E. P. KELSALL

The Appraisal of Intelligence. By A. W. HEIM. London: Methuen, 1954. Pp. vii + 171. Price 12*s*. 6*d*.

In this book Dr Heim makes a violent onslaught upon contemporary ideas about the concept of intelligence and methods of testing it, and she presents her own views on these subjects.

Her own views are, briefly, that it is better to speak of 'intelligent activity' than of 'intelligence', "to avoid the suggestion of some one trait which an individual simply possesses, to a greater or lesser extent" : 'intelligent activity' consists in grasping the essentials in a given situation and responding appropriately to them; and she thinks that research should be concentrated upon what are 'essentials' and 'appropriate responses'. Probably many psychologists would agree that, at present, research into intelligence is unbalanced, too much effort going into purely statistical studies along factorial lines, for instance, compared with effort put into qualitative studies of 'intelligent activity'. There is, of course, a good reason for the imbalance—there is a market for intelligence tests and, indirectly, for statistical studies of these tests—but a well-written exposition of views like Dr Heim's and of how research along new lines might be conducted would be very welcome. Unfortunately Dr Heim's exposition is not well-written.

Indeed her thinking is so seldom cogent and so frequently very loose that her destructive criticism will have little impact and her constructive criticism may be overlooked.

For instance, we find Chapter 3 entitled "Some current definitions of intelligence": in paragraph 2 our natural expectations are disappointed when we are told that only two definitions (Raven's and Heim's) will be discussed (*i.e.*, 'some' = 2). Raven's definition is soon disposed of; then Dr Heim gives hers: then she says: "This can scarcely claim to be a definition and it is certainly not precise" (at this stage we deduce that 'some' = 2 = 1); finally she goes ahead and uses what "can scarcely claim to be a definition" as a definition. Again, at the end of Chapter 6, on "Test 'Reliability'", we find this statement: "The term 'reliability' has been shown to be applied to a variety of methods and a variety of notions having relatively little in common. Each has its uses but to employ the same word for all these (and, occasionally, yet other) concepts seems a false economy. Since the practice has led to confusion, the banishment of the term 'reliability' is advocated and in its place the term 'consistency' is offered". How does this renaming process cure the alleged evils? Immediately following this we have Chapter 7 headed "Unreliability in mental testing". Dr Heim does recognise the awkwardness of such a title following her last statement and says: "Since I have rejected the term 'reliability' it may seem curious to devote a chapter to a discussion of types of 'unreliability'". It *is* curious, and the more so because the author recognises the incongruity and still allows it. Such examples could be multiplied. Those quoted illustrate common faults in the book; the tendency to consider only the views of one or two people when the author's intention seems to imply (and her sweeping conclusion should demand) a more extensive analysis; the tendency to recognise a fault or an awkwardness in the text, and to do nothing to remove it, and even to persist with it; and the occasional wild absurdity.

The book is intended primarily "for those members of the community whose work brings them into contact with psychological tests or with their results: school teachers and other educationists, careers masters and other vocational counsellors, psychiatrists and psychiatric social workers, personnel managers and employment officers". It is quite unsuitable for such readers.

EDWARD ELLIOTT

Labor-Management Relations in Illini City. By W. ELLISON CHALMERS, MARGARET K. CHANDLER, LOUIS L. Mc QUITTY, ROSS STAGNER, DONALD E. WRAY, MILTON DERBER, and 25 research assistants. Volume 1—The Case Studies. Pp. xlii + 809. Price $10.00. Volume 2—Explorations in Comparative Analysis. Pp. x + 662. Price $7.50. Illinois: Institute of Labor and Industrial Relations, 1954. Price for the two volumes $15.00.

These two large volumes include the names of nine professors, in addition to those quoted above, amongst their credit titles. On this evidence alone they suggest an important contribution to the study of industrial relations. Even apart from the distinction of the contributors, these studies must command respect for their scope and range. In a period of six years the research team completed a sociological analysis of 'Illini City' and carried out detailed historical researches and attitude surveys in eight establishments covering five industries in the city. On finding the uniformity of methodological approach in each of the case studies, anyone who has attempted team research in the social sciences will feel considerable admiration for the co-ordinators of the project. It was one of the aims of the research to marry the efforts of a number of scientists from several social science disciplines, for it was held that some of the major limitations of previous studies arose from the fact that they had been made separately by economists, or sociologists, or psychologists. While it is clear that complete integration of the three disciplines is still remote, these studies are particularly satisfying in presenting a complex problem in its broad complexity instead of isolating facets of the material to suit the requirements of single abstracted theories. The multi-disciplinary approach is not, of course a new idea; hitherto, however, it seems usually to have resulted in inter-disciplinary reproach rather than team achievement.

Volume 1 sets out the evidence from the field studies. It opens with an historical, sociological survey of the city. This provides welcome background material for the

British reader who may sometimes be puzzled by the implications of residential districts in American municipalities. From the description of the development of 'Illini', the analysis of the voting record of its inhabitants, and from the description of the architectural diversity of its various wards, one can see the general form of its class structure and can decide which is 'the right side of the tracks' and why. A public opinion survey of a 200 person sample of citizens gives a picture of general attitudes to labour relations issues in the community. Two further samples of 114 Union members, and 87 'management persons who dealt with unionised workers' were also polled on the same issues to obtain a measure of the divergencies of views held by special interest groups. A description of the development and activities of the Chamber of Commerce shows the extent of civic activity and local autonomy in industrial relations issues.

There follow the five case studies: a large grain processing establishment, an engineering concern, several small garment manufacturing establishments—the two largest of which receive detailed analysis, the trucking (road transport) industry in the city—two establishments again being picked for detailed study, and the building industry in the city, where again two major establishments receive major attention. Each case study includes an historical retrospect, a survey of the essential features of the industry, a description of the trade unions associated with the industry—their organisation and inter-relations, attitude surveys of workers, union officials, and management personnel, and details of the economic background of the industry and its employees. In each case three main factors are dealt with—attitudinal climate, economic status, and extent of Union influence—for it is the inter-relationship of these three that forms the theoretical foundation of the entire research.

The case studies are—inevitably—lengthy, as they are designed to document their subjects as fully as possible, but they are on the whole admirably lucid and refreshingly free from pretentious social science jargon. They should make admirable teaching material, for despite their American setting there seems to be a great deal in them which could be generalised to other highly industrialised countries. Tutors should be warned, however, that it will take students some 17 hours to read Volume 1 alone, even if they work at a steady 300 words a minute—a fastish speed for material of this complexity and one which does not allow for any note taking or cross-checking of figures!

Volume 2 is of rather more specialised interest. It has two main intentions—first, to draw together the evidence from the field studies in an endeavour to test a number of hypotheses about the inter-relations of attitudinal climate, economic status, and the extent of Union influence; and secondly to compare and appraise various methods of analysis of the quantitive and qualitative data collected in the field studies.

On the whole the hypotheses which were selected for testing had a fairly high degree of *a priori* plausibility. Thus where they have been confirmed one feels satisfaction but no great surprise—*e.g.* "If management and union leaders better understand each others' goals, the attitudinal climate tends to be more harmonious." But in several instances the *a priori* plausible was not found to be sufficiently strong in fact to emerge with a reasonable level of statistical significance from the field data—and although the tendency is usually in the expected direction, and allowing for the multi-variate complexity of the issues, it would appear either that *a priori* speculation has tended to give undue weight to issues which in fact have minor influence, or the method of analysis has pulled a boner. It is these discrepancies which provide much of the most interesting material for reflection.

The sections on methods of analysis include a chapter by Professor McQuitty in which he discusses and compares three main types of quantitative analysis—correlation, cluster, and pattern analysis—and these are then illustrated with the material from the field studies. Unfortunately, however, the comparison of these methods seems, to this reviewer, to be inconclusive because of the limitation that the correlations are rank order co-efficients based on only eight establishments, and these co-efficients are dependent on linear relationships between the variables. It would seem that many more case studies will be required before adequate matrices can be built up which will provide conclusive tests of the different methods of analysis. This, however, is not a counsel of despair, for it is clear that the methods employed here can be carried straight over—possibly even straight back—to the study of individual personality instead of 'organisation types'. Whether such testing is really necessary, however, is arguable—for the methods discussed seem, to this reviewer, to be much less logically discrete than Professor McQuitty appears to hold. The section is, however, a valuable survey of the general issues in this field. One point seems, however, to have got rather lost in the transition from Volume 1 to Volume 2. In the case studies one cannot help being impressed with the frequency with which some important change is attributed to the impact of a single decisive individual—a reactionary employer—an incompetent Union official—a progressive business agent—and so forth. The quantitative methods adopted for the analysis in Volume 2 do not seem to be well adapted to dealing with these apparently important, although intrusive individuals. This fact may have some bearing on the occasional failure to confirm a plausible hypothesis from the quantitative data.

The two volumes form an important and impressive achievement. Admirably objective about topics which are politically red-hot, careful and modest in the presentation of tentative conclusions, thoughtful and painstaking over methodological issues, and not least, in giving evidence of harmonious co-operation in an inter-disciplinary research, they mark a real advance in the study of industrial relations.

JOHN D. HANDYSIDE

Minds and Machines. By W. SLUCKIN. London: Pelican Books, 1954. Pp. 224. Price 2*s*.

This is an interesting book on an important subject. The use of electronic machines has greatly increased since the war, and continues to increase, so that we ignore at our peril their potentialities and their implications for psychology. However, the writings dealing with such machines and their theory are highly technical, and the ordinary reader finds himself faced with formulae which are outwith his competence, and terms which look familiar but are in fact used in a specialized and unfamiliar way. So he requires a non-technical guide to initiate him into the mysteries of the subject, to explain what it all means, what it is all about, and what is the purpose of it all. The author is well qualified to write about his subject, being an engineer as well as a psychologist, and the book can be confidently recommended to the ordinary reader.

Mr Sluckin, after a long introduction, gives an account of the principles of operation of computing and analogue machines, and what they can do. In this section the author seems quite at home with his subject and provides a very helpful and much-needed summary of the field. Then he describes the working of the human nervous system and its similarities to the working of electronic machines. This is a more controversial matter, and Mr Sluckin proceeds cautiously. He goes on to show and comment upon the similarities between humans and machines in perception, memory, learning and problem solving. Although this section is interesting, the author appears to develop some doubts as to what he is really driving at. Finally he grapples with the questions "Can machines be said to think?", "Can we describe the functioning of such machines as purposeful and teleological?", and "What metaphysical problems are raised by writings on cybernetics?" In these last five chapters the author is in deep waters indeed. The account flags and loses its teleological orientation, until it finally reaches an equilibrium situation resembling that of a dog chasing its tail.

As new sciences are born and accumulate a body of knowledge, and the results of scientific endeavour become more important in their effects on everyday life, there grows up a need for the sciences to engage in public relations work. Someone is required to explain the science, its content and its theories, firstly to the general public, and perhaps more important, to the practitioners in the other sciences. This public relations work is fraught with dangers. It is understood that the writer is always liable to be charged with too much generalizing and oversimplification, by the practitioners of the science. On the other hand, the public may rightly complain if they are given an account of a science which reads more like an initiation into a priesthood and its mysteries than a description of a guild of craftsmen with usable tools and repeatable procedures. This is especially true if the public addressed comprises scientists of the other sciences. If the account leans too much to the esoteric, other scientists are likely to conclude that the tools are magic and are of no use to scientists other than the wizards who wield them. At one time, quite considerable effort was put into the task of demarcating what material scientists of a given kind must or must not study, and building up the mystery of what they did study. Nowadays, there is much more appreciation of the value of an interdisciplinary approach, where the tools of several sciences can be simultaneously applied to a problem, and of the value to craftsmen of having some knowledge of, and being able to some extent to use each other's tools.

Mr Sluckin's book, considered as a popular account directed to the informed layman, is good. It is sober in its presentation, cautious in its judgments, and unlikely to mislead anyone into making a religion out of cybernetics. But the psychologist, or industrial psychologist, looking for tools to use in his science, will be somewhat disappointed. Possibly the main omission for the industrial psychologist is any discussion of the implications of 'information theory' for his subject. Mr Sluckin does devote two pages to information theory. He mentions that there are three measures of amount of information, structural, metrical and selective, defines them and gives some indication as to where each type may be followed up. But he says nothing about the possible uses of information theory as a research tool in psychology. Industrial psychologists have many problems of inspection and display to solve, problems of work study, problems of intelligibility of communication, all of which information theory might help to solve. Some of this help may come, of course, by way of statistics, expressed in other language. But this book, one feels, might have indicated how information theory is connected with these problems, how its terms differ in use from the common usage, and how little it has to do with 'communication in industry'. We are told that that which is most unexpected yields most selective information; but in the ordinary run of things what people in industry want is expected information, in an entirely different sense of both words.

It might also be maintained, from the point of view of a psychologist, that Mr Sluckin has chosen to exhibit spectacular similarities rather than useful tools. We are told much about the 'model organisms' of Grey Walter and W. R. Ashby, and the extent to which they can imitate human behaviour, and are drawn into discussions about whether they can be said to think, or function teleologically. The student of human mental activity, after considering Mr Sluckin's discussion, might well see himself as faced with the problem of defining thought in such a way that machines cannot be said to think, whilst the experimental psychologist, having studied humans and animals more or less as 'black boxes' which respond in certain ways to stimuli, might see himself as faced with the problem of whether to extend his study to include the behaviour of electronic black boxes which behave in comparable ways.

The answer to this seems to be that such questions as "Do machines think?" and "Do they exhibit purposive behaviour?" are quite useless and irrelevant for a scientist to ask, however challenging they may be to a philosopher, and are mere side issues, just as the question "Do animals think?" proved to be fifty years ago. A psychologist may study the gramophone, television or radio as media for communication, but what he studies as a psychologist is human behaviour as mediated by these instruments, not the instruments as important in themselves. So the physical engineering in these model organisms need be of but little interest to psychologists, however teleologically they may seem to behave and however much they may seem to think.

If one is looking for research tools, the important thing is the theory behind these machines. According to MacKay, who is much more sanguine than the author of this book about the possible contribution of information theory to psychology, the relevant question is "To what extent can a suitably designed artefact embodying only combinations of physical processes now known, provide evidence of mentality comparable with that acceptable from human beings?"

The language of information and control, suggests MacKay, is conceptually intermediate between psychology and physiology. The theory of an automaton designed to be a research model of the human organism considered as an information flow system, might therefore serve as a useful conceptual bridge between two fields of data. It could provide a common testing-ground and a common language in which hypotheses might be framed and tested and progressively refined with the help of clues from both sides. The physical engineering of the automaton would be of little interest, as also would what could be imitated and how. What would be of interest would be the development of a hypothetical map of the information flow of the human organism which would suggest questions testable by both psychological and physiological experiment. This would offer a new approach to the explanation of human behaviour in terms of communication and control.

We have little word from Mr Sluckin on the possibilities of such an approach, although we are suitably reminded of the shortcomings of the black box approach to the explanation of humans. Mr Sluckin considers machines in relation to McDougall's seven marks of behaviour or action of living things. It would probably have been more useful and interesting to psychologists to have had the theory of an automaton as suggested by MacKay considered as an explanation of human behaviour, comparing it with other explanations offered by psychologists. Hull, for example, who is not once mentioned in this book, can be considered as having made an early attempt at using the language of physics and neurology to provide a comprehensive explanation of human behaviour; but it is quite possible that his systematic behaviour theory may be supplanted by a new type of theory developed by using the theory of automata and the hypothetico-deductive method. The theory suggested by MacKay shows interesting resemblances to Hull's theory, although developed quite independently.

As these comments indicate, this book is not the last word on the impact or the implications of cybernetics for psychology. To cover this subject thoroughly a different approach would have to be adopted, and different questions answered, in a book directed more specifically to psychologists. But Mr Sluckin's book is not specifically directed to psychologists, so it is hardly to be expected that he will answer their particular questions. It should be sufficient that his book will adequately fulfil the function and inform the audience for which it is intended.

DAVID C. DUNCAN

Motivation and Morale in Industry. By MORRIS S. VITELES. London: Staples, 1954. Pp. xvi+510. Price 50*s*.

There can be no doubt whatever that this new book by Dr Viteles will become a standard work as quickly as did his *Industrial Psychology*, published in Britain in 1932. It is by far the most comprehensive and most thoroughly documented work of its kind, and there seems only the remotest chance that it will have a rival for many years to come. It is worth having for the references alone, many of which, among the 1400 or so, will be unfamiliar to readers over here.

The weaknesses of the book, such as they are, stem from what must have been for Dr Viteles an awkward dilemma. Should he write for the psychologist, and do something substantial about tidying up the messes we have got ourselves into on the theoretical side, or for the man of business, who is rarely able to raise much enthusiasm for the dissection and discussion of theoretical issues? He has finished up somewhere in between. The best bits of the book are, in the reviewer's opinion, those labelled 'Experimental Studies' (90 pages) and 'Employee-Attitude Surveys' (160 pages). Throughout, appropriate attention is given to the work of British investigators, notably Wyatt and Marriott.

No teacher or student of occupational psychology is likely to regret the purchase of *Motivation and Morale in Industry*. Among the others who should have it, apart from the libraries of university psychological departments, are the libraries of technical colleges and all those other places where management courses of any kind are run.

ALEC RODGER

Research Methods in the Behavioral Sciences. Edited by L. FESTINGER and D. KATZ. London: Staples, 1954. Pp. xi + 660. Price 50*s*.

It would not be very difficult for British psychologists to produce parallels to most of the collections of essays published by their American colleagues, if they had time. But here, we must admit, is one that could not have been produced outside the United States. It presents, with outstanding competence, an exposition and discussion of research methods which are rapidly becoming part and parcel of the equipment of many American investigators, but which are still to a large extent unknown—or at least untried—elsewhere. It is an outcome of collaboration between a group of specialists, mostly social psychologists, at the University of Michigan, and (as the Editors explain in their Foreword) it aims to help in the codification of research techniques and to give graduate students in the field some understanding of the principles and procedures of modern methodology. "The criterion for inclusion of methods was the degree of relevance to the problems of social psychology, and the criterion for exclusion was the availability of knowledge about a technique already standardised in another field." Thus, factor analysis, though considered useful, is out; but the quantitative analysis of qualitative material is in.

The planning and editing of the book have been remarkably well done. After a 12-page 'overview' (Newcomb) comes Part 1, on research settings, with chapters on the sample survey (Campbell and Katona), field studies (Katz), experiments in field settings (French) and laboratory experiments (Festinger). Part 2, on procedures for sampling, consists of one chapter (Kish). Part 3, on methods of data collection, has chapters on objective observation (Peak), the use of documents, etc. (Angel and Freedman), the collection of data by interviewing (Cannell and Kahn) and the observation of group behaviour (Heyns and Zander). Part 4, on the analysis of data, is composed of chapters on the analysis of qualitative material (Cartwright), theory and methods of social measurement (Coombs) and distribution-free statistical methods and the concept of power efficiency (Smith). Part 5, on the application of research findings, is another single chapter (Likert and Lippitt). Each part has a short, meaty, thoughtful introduction; and each chapter has a good bibliography. Throughout, the writing is clear, direct and free from esoteric jargon.

ALEC RODGER

Selection for Industrial Leadership. By A. G. ARBOUS. London: Oxford University Press, 1953. Pp. xiv + 180. Price 30*s*.

Apologies are due to the author for the belated appearance of this review, which is the fault neither of the Editor nor of the present writer. For the book certainly merits the careful attention of readers of *Occupational Psychology*, and of all concerned with the selection of managers, executives, and officers in the Services. The National Institute for Personnel Research in South Africa is very active in the field of applied psychological research, and one of the lines in which Mr Arbous has specialised is the development of statistical techniques for expressing the amount of wastage resulting from different methods of personnel selection, and the dependence of such wastage on the proportion of candidates selected, the proportion of these who are successful, and the correlation between the selection procedure and success. Arbous's techniques may seem rather elaborate and technical for everyday application, but they do have great advantages in demonstrating quantitatively the effects of any change in conditions, such as raising the standards of acceptance. Their use is here illustrated by an account of an investigation into the selection of industrial executives for high-grade administrative work in a South African Corporation. A battery of intelligence, aptitude and other tests, together with group procedures, was applied, and careful follow-up showed that the accuracy of selection was very much superior to that of ordinary interview methods. The book also contains a valuable survey of the literature on group methods from WOSB, CISSB, OSS and other sources, pointing out the weaknesses of validatory evidence to-date.

P. E. VERNON

Some Young People. By PEARL JEPHCOTT. London: George Allen and Unwin, 1954. Pp. 168. Price 12*s.* 6*d.*

This book is a summarised account, by one of the research team, of a 'study, sponsored by King George's Jubilee Trust, of adolescent boys and girls in three areas, with special reference to their membership of youth groups'. Like Miss Jephcott's earlier books, *Girls Growing Up* and *Rising Twenty*, it presents a graphic account of some of the difficulties, such as paucity of home facilities, and intellectual apathy in the local adult community, which confront so many of our young people. (Unlike the earlier books it is concerned with peace-time, and with rural as well as urban areas.) At the same time it high-lights a number of factors—inadequate accommodation, dreary and unkempt appearance, lack of material facilities such as tools and sports equipment, and, by no means least, the shortage of really well-qualified youth leaders—which appear to lessen the attraction of youth groups for these intellectually deprived adolescents.

If the position revealed, or perhaps one should say confirmed, by this pioneering large-scale study holds true for most of the country—and this seems a reasonable assumption in view of the care taken to choose representative districts for investigation—it would appear that youth work is a field in which our psychological knowledge (or is it our practice?) is far from adequate. For the enquiry suggested strongly that the failure of youth groups of any kind to make effective contact with about two-thirds of their potential clientèle was due to a considerable extent to their failure to take proper account of the needs of these young people, and of the ways in which these needs change with increasing social maturity. The more successful groups appeared to meet the adolescent's need for activity, though girls were less well provided for than boys. The severest shortcomings seemed to be on the psychological side. Perhaps what most affected group membership was incomplete understanding of the social pressures to which adolescents are subject. While thrice-weekly visits to the cinema might be approved at home, attendance at a youth group which contributed nothing obviously useful to occupation or marriage might be regarded as a waste of time. Only a minority of groups catered for both sexes, a strong deterrent both to the young woman in serious search for a 'boy', and to courting couples, who might have been glad to attend, together, a group some of whose activities were directed towards better home-making. It was thought that a greater measure of self-government would have attracted, or maintained attraction for, some proportion of the young people who, in their struggle to achieve adulthood, rebelled against an organisation which savoured in any way of school, or which drew attention to the adolescent status of its members.

Detachment from reality was shown in the recruiting arrangements of some groups. Insufficient use was made of existing facilities for making contacts with young people and their parents; little attraction was offered to small gangs or informal teams; there was no appeal to 'me *and* my pal', which could conceivably have brought in some of the socially insecure girls said to be afraid lest joining a group would lead to the loss of their only real companion; and too often it seemed that the leaders were too busy 'organising' to give sufficient attention to helping new members over the critical period of induction—it was 'noted time and again . . . how frequently a youngster responded to and throve on the interest of some older person'. It is suggested, at least by implication, that leaders could be more effective, and less busy, if their efforts were devoted less to 'doing' and more to providing opportunities for members to make their own arrangements (and so, incidentally, satisfy another set of psychological needs). Little assistance was received by most of the leaders from the adults of the community their groups served.

To summarise, this book, which should surely be read by all concerned with the welfare or employment of youth, suggests that many of the personnel problems of youth organisations are very similar to those of employing organisations. Presumably they could be tackled in much the same way—by proper attention to 'working conditions', recruitment, selection, induction, training for leadership, and participation in management.

DERMOT STRAKER

ADDENDUM

DR WILLIAM STEPHENSON's *The Study of Behaviour: Q-Technique and Its Methodology*, reviewed in the January, 1955, issue of this Journal, costs 56*s*. 6*d*. The British agent for the publisher, the University of Chicago Press, is the Cambridge University Press.

Other Books Received

The following books have been received. Their inclusion in this list does not preclude their review later.

A Dictionary of Pastoral Psychology. By VERGILIUS FERM. New York: Philosophical Library, 1955. Pp. xii + 336. Price $6.00.

Ageing Men in the Labour Force: Third Report on the Later Working Life in the Building Industry. By F. LE GROS CLARK. London: Nuffield Foundation. Pp. 18. A limited number of copies available free of charge.

An Introduction to Mental Measurement and its Applications. By C. A. RICHARDSON. London: Longmans, Green, 1955. Pp. viii + 102. Price 8*s*. 6*d*.

Science and Social Action. By W. J. H. SPROTT. London: Watts, 1954. Pp. viii + 164. Price 15*s*.

Annual Review of Psychology, Volume 6. Edited by CALVIN P. STONE and QUINN MCNEMAR. Stanford: Annual Reviews, 1955. Pp. x + 518. Price $7.50.

The Vocational Aspect of Secondary and Further Education. Edited by A. J. JENKINSON and W. A. SKINNER. Volume 6, No. 13, Autumn, 1954. Bolton Technical College. 1954. (Journal.)

A New Era of Opportunity. By FRANCIS B. WILLMOTT. London: Saint Catherine Press, 1954. Pp. xviii + 136. Price 15*s*.

The Function and Training of Mental Nurses. By A. N. OPPENHEIM. London: Chapman & Hall, 1955. Pp. 86. Price 12*s*. 6*d*.

Guide Pratique du Medecin d'Orientation Professionnelle. By MARIE PARMENTIER-BELOUX and others. Paris: Masson, 1955. Pp. 264. Price 1,400 *fr*.

Working Class Anti-Semite: A Psychological Study in a London Borough. By JAMES H. ROBB. London: Tavistock Publications, 1954. Pp. xiv + 240. Price 15*s*.

Group Cohesiveness in the Industrial Work Group. By STANLEY E. SEASHORE. Ann Arbor: University of Michigan, 1954. Pp. vi + 108. Price not stated.

Training Managers in the Public Services. By F. BRAY AND OTHERS. London: Allen & Unwin (for the Royal Institute of Public Administration), 1955. Pp. (iv) 84. Price 7*s*. 6*d*.

Some Principles of Training Applied to the Retail Trade. By ELSE HERZBERG. London: Institute of Personnel Management, 1955. Pp. 24. Price 3*s*. 6*d*.

A Handbook of Industrial Welfare. By R. R. HOPKINS. London: Pitman, 1955. Pp. vi, 258. Price 17*s*. 6*d*.

Partners for Health. New York: THE NATIONAL HEALTH COUNCIL, 1955. Pp. 40. Free of charge.

English Social Differences. By T. H. PEAR. London: Allen & Unwin, 1955. Pp. 318. Price 18*s*.

The Colour Problem. By ANTHONY H. RICHMOND. London: Penguin Books, 1955. Pp. 372. Price 3*s*. 6*d*.

The Learner in Education for the Professions as seen in Education for Social Work. By CHARLOTTE TOWLE. Chicago: University Press. London: Cambridge University Press, 1955. Pp. xxvi, 432. Price 56*s*. 6*d*.

OCCUPATIONAL
PSYCHOLOGY

JULY 1955 VOLUME 29: NUMBER 3

The Relative Importance and Urgency of Certain Human Problems in Industry

SUMMARY

The enquiry described here was planned and carried out by the National Institute of Industrial Psychology as a pilot study to discover how much information on the relative importance and urgency for investigation of different human problems in industry could be obtained through the use of a questionnaire distributed to industrial companies.

Despite the fact that the companies approached could be regarded as more 'advanced' than the average, 50 per cent made no reply, and only 40 per cent completed the questionnaire.

There were two versions of the questionnaire; one asked for information in terms of relative importance of the problems listed, the other in terms of urgency of their need for investigation. The questionnaires were filled in by allotting a rating on a four point scale to each item. In addition, free comment on the three most important or most urgent problems of the organisation and of large and medium and small organisations in general was invited. The analysis is described.

A striking feature of the results is their similarity when analysed to reveal differences which might be linked with size of firm or the nature of the duties of the executive completing the questionnaire. In this group there was little evidence of a tendency to regard a firm's major problems as peculiar to itself. The problems given greatest weight were those concerned with attitudes, motivation, relations with other members of the organisation. The interpretation of this result is discussed. Other topics ranking high on the list included payment, evaluation and promotion, production methods, design of equipment and the ageing worker.

Because of the selected nature of the sample generalisation to industry as a whole is not legitimate, but the enquiry has provided some guidance, pending the production of better-based evidence, on the planning of future research on human problems of industry.

THE PROBLEM

There has been since the war a relatively large increase in the amount of scientific research devoted to human factors in industrial productivity. The subjects studied have in most cases been selected by the research workers concerned as ones of importance to industry or as matters in which the research workers were particularly interested. This may, in part at least, be due to the fact that systematic attempts have not been made to discover what questions industry regards as of particular importance, or on which it feels a special need for new knowledge. It may also be connected with difficulty found in gaining acceptance for the knowledge derived from these researches. It must be added that it is not easy to obtain from people in industry the formulation of a human problem in terms which permit the scientific worker to draw up a soundly organised plan for a research project.

PROCEDURE

It seemed worthwhile to carry out a pilot study to discover what kind of information on the relative importance and urgency of investigation of industry's human problems could be obtained from companies through the use of a questionnaire. It was therefore decided to send a letter explaining the nature of the inquiry with the questionnaire to a group of establishments which were Corporate Members of the Institute. Since it was possible to draw up a questionnaire in terms of either the relative importance or the relative urgency of investigation of the different human problems, it was decided to use the two forms in the hope of being able to compare the results. In other respects both the letter and the questionnaire were the same. The establishments were listed in alphabetical order, and the "Importance" and "Urgency" versions allotted alternately. Anonymity in the treatment of the results was guaranteed. The questionnaires were sent out in February, 1954.

The questionnaire analysed the different human problems under nine main section headings, and broke down each section heading into a number of topics ranging from three to nine. The recipients were asked to rate every section heading and every topic on a defined four-point scale. At the end of the questionnaire definitions were requested of the three most important problems in the recipient's own organisation and in large, medium and small organisations in general. A copy of the questionnaire is given in the appendix. The response is summarised in Table 1:

TABLE I

	IMPORTANCE VERSION	URGENCY VERSION
Main body of questionnaire filled in completely or with minor omissions only	40	42
Main body of questionnaire filled in for topics only	—	1
One or more "open-ended" questions only filled in	1	1
Letters offering general opinions but questionnaires not completed	4	3
Letters regretting inability to co-operate	4	3
No Reply	56	55
Questionnaires sent out	105	105

ANALYSIS OF RESULTS

When considering the result obtained from a questionnaire which involves the use of ratings, it has to be remembered that there can be no certainty that the different respondents in fact attach the same meaning to the definitions set out for each rating. Consequently a four-point scale was used in this instance in order to distinguish major from minor matters by considering four-star and three-star ratings as one group, and two-star and one-star ratings as a second group.

Factors which may have affected the importance or urgency attributed to different matters are: (i) the industry; (ii) the function of the executive completing the form; (iii) the size of the organisation. The ratings given to the various sections and topics were found to differ in some respects on the two versions of the questionnaire; the distribution of the two versions had therefore to be considered. The numbers were not large enough to permit any analysis of the influence of industry, but the data were examined for any relationships between the version of the questionnaire used, the function of the executive completing it and the size of the firm.

The executives completing the form comprised forty-eight concerned with the personnel function, either as Personnel Managers, Personnel Officers, Staff Managers, or Education and Training Officers; seven in charge of production—Works Directors, Works Managers, Production Managers; twenty-four in other functions, including Managing Directors, General Managers, Company Secretaries. Three questionnaires were said to have been completed in collaboration between two or more executives. In the analysis, therefore, the responses given by Personnel Managers and the like were compared with the responses of all others who filled in the questionnaire.

The establishments were classified as Large, Medium or Small, according to the number of employees, on the following basis:

15 Small firms of up to 500 employees
25 Medium firms of between 501 and 2,000 employees
42 Large firms of over 2,000 employees.

For analysis purposes the Small and Medium firms were grouped together.

The interrelationships between the versions of the questionnaire, the function of the official completing it and the size of the organisation are shown in Tables II, III and IV.

TABLE II

	LARGE FIRMS	SMALL AND MEDIUM FIRMS	TOTAL
Urgency Version	20	22	42
Importance Version	22	18	40
Total	42	40	82

TABLE III

	URGENCY VERSION	IMPORTANCE VERSION	TOTAL
Personnel Officers and the like	22	26	48
Others	20	14	34
Total	42	40	82

TABLE IV

	LARGE FIRMS	SMALL AND MEDIUM FIRMS	TOTAL
Personnel Officers and the like	34	14	48
Others	8	26	34
Total	42	40	82

Tables II and III show no significant relationship but in the case of Table IV there is a tetrachoric correlation of 0.66. Consequently it is not possible in considering results given by personnel officers and others, and results from large and other firms, to ascribe differences to the function of the official completing the form or to the size of the organisation when the differences are of the same kind in both cases. The 'Urgency' and 'Importance' versions of the questionnaire can, however, be compared without complications, and if comparisons are made between different kinds of executive and different sizes of firm the 'Urgency' and 'Importance' versions can be treated as one group.

Table V gives a comparison of the three and four star ratings given to each main section heading in the 'Importance' and 'Urgency' versions of the questionnaire. It shows that on only three headings were there differences of any significance, namely the Field of Relations and Communications, the Disciplinary Field and the Field of Attitudes and Satisfaction. In all three cases those completing the 'Urgency' version gave more weight to the subject than did those filling in the 'Importance' version.

TABLE V

COMPARISON OF 3 AND 4 STAR RATINGS ALLOTTED TO MAIN SECTION HEADINGS IN THE IMPORTANCE AND URGENCY VERSIONS OF THE QUESTIONNAIRE

	IMPORTANCE VERSION	URGENCY VERSION
Physical Conditions and Working Methods	11	13
Personnel Organisation	19	19
Special Categories of Employee	5	4
Welfare	6	2
Relations and Communications	23	36
Wages and Salaries	23	25
Health and Safety	5	7
Discipline	10	25
Attitudes and Satisfaction	22	39

In order to compare the results given by large and other firms and those of personnel managers and other executives, the two forms of the questionnaire were pooled. The only sections on which significant differences were found were the Disciplinary Field and Personnel Organisation; Discipline received lower ratings from Personnel Officers and large firms; because of the relationship shown in Table IV it is impossible to ascribe the differences to one or the other factor. In the case of Personnel Organisation there is no difference between Large and other firms, but a positive difference between Personnel Officers and other executives—the former attach more importance to this field than do their colleagues—a not unexpected result.

The ratings allotted to the different topics within the section headings were compared for the 'Importance' and 'Urgency' versions; details are shown in Table VI. Topics under the section heading of Relations and Communications (for which the Urgency Version received higher rankings than the Importance version) repeated this pattern. The same tendency was shown in the topics under the Disciplinary section (with the exception of 'Absenteeism', where the Importance version provided more emphasis than the Urgency version), and in the topics under the section headings of Attitudes and Satisfaction. Sections where topics showed a marked difference, though the section heading did not, were:

(a) The Field of Physical Conditions and Working Methods, where 'Design of Equipment' and 'Production Methods' were given more weight in the Urgency version than in the Importance version.

(b) The Field of Special Categories of Employee, where 'Ageing Workers' were given more weight in the Urgency version.

(c) The Wages and Salaries Field, where 'Other Forms of Giving Workers a Share in the Value of the Company's Production or Profits' and 'Financial Incentives for Supervisors and Managers' were both given more weight in the Urgency version, but where 'Systems of Payment by Results' was given greater weight in the Importance version.

TABLE VI

COMPARISON OF 3 AND 4 STAR RATINGS ALLOTTED TO TOPICS IN THE IMPORTANCE AND URGENCY VERSIONS OF THE QUESTIONNAIRE

	IMPORTANCE VERSION	URGENCY VERSION
Heating, Ventilating, Lighting and Colour	6	3
Design of Equipment	11	21
Production Methods	18	23
Selection and Allocation Methods	10	12
Training Schemes	21	22
Evaluation and Promotion...	23	21
Records and Record-Keeping	2	4
Juveniles	10	10
Ageing Workers	14	23
Married Women	4	1
Disabled	5	4
Canteens	12	2

TABLE VI (CONTINUED)

	IMPORTANCE VERSION	URGENCY VERSION
Cloakrooms and Lavatories	3	1
Pensions, Sick Benefits, Staff Welfare Funds, etc.	6	5
Social and Sports Clubs, etc.	3	1
Relations between Managers	13	18
Relations between Managers and Supervisors	14	23
Relations between Managers and Workers	8	26
Relations between Supervisors	4	17
Relations between Supervisors and Workers	11	27
Relations between Supervisors and Shop Stewards	7	22
Relations between Worker and Worker	3	12
Relations between Worker and Trade Union	4	18
Relations between Management and Trade Union	3	12
The Wages Hierarchy	13	11
Systems of Payment by Results	23	14
Other Forms of Giving Workers a Share in the Value of the Company's Production or Profits	8	23
Financial Incentives for Supervisors and Managers	11	26
Merit Rating	8	11
Industrial Diseases	4	8
Accidents and their Causes	11	12
Industrial Medical Service	2	1
Improvement of Health in Industry	3	8
Absenteeism	15	8
Unpunctuality	13	15
Attention to the Job	8	20
Quality of Work	10	20
Determinants of Satisfaction	20	38
Methods of Assessing Attitudes and Satisfaction	12	33
Resistance to Change	24	29
Identification with the Organisation	21	29

A comparison was made, pooling the results of the Importance and Urgency versions, of differences on the topic ratings related to size of the firm. Out of 41 topics only eight showed a difference of any significance; as a result this analysis was not repeated to compare Personnel Officers and other executives. Because of the picture presented by Table IV these differences cannot be positively attributed to the size of the firm. Large firms gave more weight than medium and small firms to the following topics:

Production Methods
Ageing Workers
Disabled
Relations between Supervisors and Workers
Relations between Supervisors and Shop Stewards
Relations between Worker and Worker
Relations between Worker and Trade Union

On one topic—Financial Incentives for Supervisors and Managers—small and medium firms placed more emphasis than large firms.

The open-ended questions which completed the questionnaire produced a wide variety of opinion about the most important or urgent problems of the firm. To these were added the comments expressed in letters from those who had not completed the questionnaire, or preferred to offer their general comments in this way. There were in all 220 comments on the matters of main import in the respondent's own firm, 175 of which could be classified under six headings as follows:

Attitudes and Satisfaction	65
Relationships and Communications	41
Evaluation and Promotion	23
Wages, Salaries, Incentives	22
Training	13
Organisation	11

The balance related to a score of different subjects.

The free comments were examined to see how respondents regarded their own major problems as being general to other firms of the same or different size. The questionnaire had asked the respondent to define the three human problems of most importance (or in most urgent need of investigation) in:

(a) his own organisation;
(b) large industrial organisations in general;
(c) medium and small organisations in general.

A definition of 'large', 'medium' and 'small' was also asked for. These definitions varied quite widely, but the commonest were:

Small: Less than 500 employees
Medium: 501 to 2,000 employees
Large: Over 2,000 employees.

(These definitions were used in the analysis reported above).

Comparing the first problem stated under (a), (b) and (c) gave the following results:

Of 59 firms which stated their own problem and the general problem of firms of their same size range (by the definition of size they themselves gave on the questionnaire, or by the general definition if no definition was stated), 35 stated the same problem. Twenty-four cited a different problem, i.e. they regarded their own case as a special one.

Of 56 firms which stated their own problem and the general problem of firms of a *different* size (defined as above), 22 stated the same problem and 34 a different problem.

DISCUSSION OF THE RESULTS

In considering the results of this inquiry the point must first be made that the data relate to expressions of opinion; it cannot be assumed that what an executive regards as primary, when he is filling in the form, is in fact the most important human problem of the organisation. Secondly the sample is a highly selected one, selected firstly because Corporate Members of the Institute tend to be companies with a more 'advanced' and 'enlightened' outlook on human aspects of industry; and secondly by willingness to take part in the inquiry. Generalisation to industry at large is therefore not justified; the results are of value mainly in throwing light on the question of how much information can be obtained by the questionnaire method, and in indicating points which seem to be of most concern to a group of executives in companies where conditions are probably above average.

It is notable that even with this highly selected group no answer at all to the request for co-operation was obtained from 111 firms out of 210 approached and only 82 questionnaires were completed. This provides another instance of the reluctance of industrial executives to fill in questionnaires for an inquiry of this sort. Any more extended investigation should not rely on this method.

Perhaps the most striking feature of the results is their similarity, when analysed to reveal differences which might be linked with size of firm or the nature of the duties of the respondent. This similarity is emphasised by the proportion of respondents who, when giving in their own words the most important or urgent matter for investigation in their own firm, stated the same problem as that they indicated to be of the greatest importance in other firms.

The Importance and Urgency versions yielded results which differed in some respects. The tendency was for the Urgency version to give higher ratings than the other. Some respondents expressed difficulty in accepting the definition of the four star rating on the Importance version, but this seems insufficient to account for the differences, which are also not consistent. In some cases, in fact, the Importance version gives the higher rating. The details can be seen in Tables V and VI.

The nature of the topics on which the Urgency version produces more emphasis suggests that most fall into two classes. First, matters of an intangible kind to which responsibility for difficulties cannot be directly attributed but on which respondents recognise the need for new knowledge. This class includes topics bearing on attitudes, satisfaction, relations and communications. Secondly there are matters, such as ageing workers, design of equipment, forms of giving workers a share in the products or profits, where immediate problems may not be very important, but which are recognised as problems for the future, on which investigation should therefore be carried out. Two topics, Systems of Payment by Results and Absenteeism, which are rated more highly on the Importance form, seem to suggest scepticism about the value of research on these thorny questions.

Provided the differences referred to in the last paragraph are not overlooked, the most convenient way of summarising the results of the Inquiry is to rank the section headings and the topics in order according to the number of four or three star ratings allotted to them, combining the results of both forms of questionnaire. This process yields Tables VII and VIII.

TABLE VII

MAIN SECTION HEADINGS RANKED ACCORDING TO NUMBER OF FOUR OR THREE STAR RATINGS ALLOTTED

SECTION HEADING	FOUR OR THREE STAR RATINGS ALLOTTED
The Field of Attitudes and Satisfaction	61
The Field of Relations and Communications	59
The Wages and Salaries Field	48
The Personnel Organisation Field	38
The Disciplinary Field	35
The Field of Physical Conditions and Working Methods	24
The Health and Safety Field	12
The Field of Special Categories of Employee	9
The Welfare Field	8

TABLE VIII

TOPICS RANKED ACCORDING TO NUMBER OF FOUR OR THREE STAR RATINGS ALLOTTED

	FOUR OR THREE STAR RATINGS ALLOTTED TO TOPICS
Determinants of Satisfaction	58
Resistance to change	53
Identification with the Organisation	50
Methods of Assessing Attitudes and Satisfaction	45
Evaluation and Promotion	44
Training Schemes	43
Production Methods	41
Relations between Supervisors and Workers	38
Relations between Managers and Supervisors	37
Systems of Payment by Results	37
Financial Incentives for Supervisors and Managers	37
Ageing Workers	37
Relations between Managers and Workers	34
Design of Equipment	32
Relations between Managers	31
Forms of giving Workers a Share in the Value of the Company's Production or Profits (other than payment by results)	31
Quality of Work	30
Relations between Supervisors and Shop Stewards	29
Unpunctuality	28
Attention to the Job	28
Wages Hierarchy	24
Absenteeism	23
Accidents and Their Causes	23
Selection and Allocation Methods	22
Relations between Worker and Trade Union	22
Relations between Supervisors	21
Juveniles	20
Merit Rating	19
Relations between Worker and Worker	15
Relations between Management and Trade Union	15
Canteens	14

TABLE VIII (CONTINUED)

	FOUR OR THREE STAR RATINGS ALLOTTED TO TOPICS
Industrial Diseases	12
Improvement of Health in Industry	11
Pensions, Sick Benefits, Staff Welfare Funds, etc.	11
Heating, Ventilating, Lighting and Colour	9
Disabled	9
Personnel Records and Record-Keeping	6
Married Women	5
Cloakroom and Lavatories	4
Social and Sports Clubs, etc.	4
Industrial Medical Service	3

It will be seen that problems of attitudes and satisfaction appear at the head of both tables, and that those of relations and communications are also prominent. It should be recognised that this result may to some extent reflect the emphasis on these questions in discussion of industry's human problems since the war; they may be 'fashionable'. Their popularity as jargon may also have had an influence. The free comments of the respondents, however, referred to earlier, which showed a preponderance which could be classified in the jargon terms, rarely used them. The commontst phrases referred to 'loyalty', 'sense of responsibility', 'resistance to change', 'interest in work'. It may fairly be concluded that this group of industrial executives attached most weight to psychological problems of the employees' attitudes, motivation and relations with fellow employees and the organisation itself.

Questions of remuneration rank high in the lists, although the prominent position of 'Financial Incentives for Supervisors and Managers' may reflect the respondent's feelings about his own financial problems.

'Evaluation and Promotion' and 'Training Schemes' which come fifth and sixth in the list of topics, do not reflect only a concern for the operative's legitimate aspirations for advancement. The free comments also stressed the importance of promotion and training schemes which would ensure the supply of competent and well-trained executives to provide the senior managers of the future.

'Production Methods' which was amplified on the questionnaire by the words 'the effects of job simplification, increasing mechanisation, conveyor pacing, group work, etc.' takes seventh place, and 'Design of Equipment' the fourteenth in the list of topics. The task situation was clearly very much in the minds of this group of executives.

It is 'Relations between Supervisors and Workers' and 'between Managers and Supervisors' which head the topics under the Relations and Communications section. These respondents seem to accept the view that the foreman is the key man in the hierarchy.

The position of 'Ageing Workers' is due to the emphasis it received in the Urgency version of the questionnaire. It reflects, perhaps, the result of discussions in the Press and elsewhere of the changing age distribution of the population, and the view that this will be a major problem of the future.

CONCLUSION

Because of the selected nature of the sample the results of this inquiry must be treated with caution. Failing better sources of information, however, they may provide research workers with some guidance in the planning of future research projects. For this outcome tribute must be paid to the collaboration of the executives who completed the questionnaire and offered helpful comments on the purpose it was designed to serve. Their part is most gratefully acknowledged.

APPENDIX

Attached is a copy of the 'Importance' version of the questionnaire. The 'Urgency' version was the same except that:

(a) The title was: "Inquiry into the Relative Urgency of Investigation of Various Human Problems of Industry".

(b) The first part of page 1 was worded as follows:

> "This form is designed to collect information from managers about the relative urgency of investigation of different human problems in industry and commerce . . .
>
> "We have attempted to classify the numerous points at which managers can find themselves faced with difficulties under nine main headings. Under each of these headings we have listed what seem to us the principal topics. What we are seeking to discover is the relative urgency of investigation of the fields covered by the main section headings, and the relative urgency of the topics within each section. You will see that the form is ruled so that it provides a box beside each section heading and a box beside each topic. What we ask you to do is to mark each box according to the code below to show your opinion of the urgency of research into the subject. The section headings are for a group of topics; the topics are items within the groups. You could quite legitimately allot a relatively low degree of priority to a section heading, and a high degree of priority to one topic within that field.
>
> XXXX—should have top priority
> XXX—needs investigation and research as soon as possible
> XX—not urgent, because already a good deal of knowledge available
> X—no need for further research; ample information available
>
> "Lastly, there is a question about your views on the three most urgent human problems as you see them. Even if you feel that filling up the first part of the form is too much of an imposition, will you please complete the last section?"

(c) The question on page 3—"How would you define the three most important human problems—the things that cause most anxiety and most difficulty—as you see them—in each of the following?" was phrased:

> "How would you define the three human problems on which you feel there is the most urgent need for further investigation and research so far as the following are concerned . . .?"

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY

INQUIRY INTO THE RELATIVE IMPORTANCE OF VARIOUS HUMAN PROBLEMS OF INDUSTRY AND COMMERCE

This form is designed to collect information from managers about the relative importance of different human problems in industry and commerce, most of which demand attention from time to time, but some of which are much more pressing than others. Conditions will obviously vary from one firm to another, and we hope that you will give us your opinion on the questions asked in the light of your own personal experience.

We have attempted to classify the numerous points at which managers can find themselves faced with difficulties under nine main section headings. Under each of these headings we have listed what seem to us the principal topics. What we are seeking to discover is the relative importance of the fields covered by the main section headings, and the relative importance of the topics within each section. You will see that the form is ruled so that it provides a box beside each section heading, and a box beside each topic. What we ask you to do is to mark each box according to the code below to show your opinion of the importance of the subject. The section headings are for a group of topics; the topics are items within the groups. You could quite legitimately allot a relatively low importance to a section heading, and a high importance to one topic within that field.

XXXX—extremely important; a real and regular headache.
XXX—difficulties frequently arise; some serious.
XX—occasional difficulties, but no serious problems.
X—never have to worry.

Lastly, there is a question about your views on the three most important human problems as you see them. Even if you feel that filling up the first part of the form is too much of an imposition, will you please complete the last section?

Section Rating .... **In the field of physical conditions and working methods**

Topic ratings .... (a) *Heating, Ventilating, Lighting and Colour.* The determination of the conditions which best facilitate the job.

.... (b) *Design of Equipment.* The principles which should govern the design of tools and machinery so that they may be most easily used by human beings.

.... (c) *Production Methods.* The effects of job simplification, increasing mechanisation, conveyor pacing, group work, etc.

Section Rating .... **In the personnel organisation field**

Topic ratings .... (a) *Selection and Allocation methods.* The effectiveness of the interview, tests and other means.

.... (b) *Training Schemes.* The principles which underlie the effective training of operatives, supervisors and managers.

.... (c) *Evaluation and Promotion.* Methods of merit-rating and staff reporting. Promotion policy.

.... (d) *Records and Record-Keeping.* The nature of records which are desirable, and effective means of keeping them accurately.

Section Rating .... **In the field of special categories of employee**

Topic ratings .... (a) *Juveniles*

.... (b) *Ageing Workers*

.... (c) *Married Women*

.... (d) *Disabled*

.... (e) *Other*—please specify.

Section Rating .... **In the welfare field**

Topic ratings .... (a) *Canteens*

.... (b) *Cloakrooms and Lavatories*

.... (c) *Pensions, Sick Benefits, Staff Welfare Funds, etc.*

.... (d) *Social and Sports Clubs, etc.*

.... (e) *Other*—please specify.

Section Rating .... **In the field of relations and communications**

Topic ratings .... (a) *Between Managers*

.... (b) *Between Managers and Supervisors*

.... (c) *Between Managers and Workers*

.... (d) *Between Supervisors*

.... (e) *Between Supervisors and Workers*

.... (f) *Between Supervisors and Shop-Stewards*

.... (g) *Between Worker and Worker*

.... (h) *Between Worker and Trade Union*

.... (i) *Between Management and Trade Union*

Section Rating .... **In the wages and salaries field**

Topic ratings .... (a) *The Wages Hierarchy.* Job evaluation.

.... (b) *Systems of Payment by Results.* The methods of fixing standard times or piece rates; effects of different incentive schemes.

.... (c) *Other Forms of Giving Workers a Share in the Value of the Company's Production or Profits.*

.... (d) *Financial Incentives for Supervisors and Managers*

.... (e) *Merit Rating*

Section Rating .... **In the health and safety field**

Topic ratings .... (a) *Industrial Diseases.* Their diagnosis, treatment and prevention.

.... (b) *Accidents and their Causes;* accident prevention.

.... (c) *Industrial Medical Service ;* first aid and treatment provisions.

.... (d) *Improvement of Health in Industry*

Section Rating .... **In the disciplinary field**

Topic ratings .... (a) *Absenteeism*

.... (b) *Unpunctuality;* arriving late; delay in restarting after rest breaks; early knock-off.

.... (c) *Attention to the job;* the rate of working.

.... (d) *Quality of Work*

.... (e) *Other sources of Friction*—please specify.

Section Rating .... **In the field of attitudes and satisfaction**

Topic ratings .... (a) *Determinants of Satisfaction.* What are the conditions leading to satisfaction and collaboration from: (i) the manager? (ii) the supervisor? (iii) the operative?

.... (b) *Methods of Assessing Attitudes and Satisfaction*

.... (c) *Resistance to Change.* How can change in policy, organisation and methods be made acceptable and introduced smoothly?

.... (d) *Identification with the Organisation.* How can collaboration and participation of all be strengthened?

Note: Dotted lines represent boxes.

How would you define the three most important human problems—the things that cause most anxiety and most difficulty—as you see them—in each of the following:

(a) *your own organisation?*

(1)
(2)
(3)

(b) *large industrial organisations in general?*

(1)
(2)
(3)

(c) *medium and small industrial organisations in general ?*

(1)
(2)
(3)

Please give your definition of 'large', 'medium', 'small'.

The industry of which your Company is a unit:……………………………………

Your Company's products: ……………………………………………………

The approximate number employed with the Company: Male……………

Female…………

Your own position in the Company:…………………………………………

Date:……………………………

The Sensory Basis of the Cheese-Grader's Skill

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Many studies have been made on the skill of craftsmen since Katz (1938) was appointed as consultant psychologist to the Research Association of British Flour Millers, to obtain some psychological understanding of the baker's methods of assessing doughs. Perhaps the most extensive programme of research has been devoted to studying the skill of the cheese-grader. It was inaugurated by Dr G. W. Scott Blair at the National Institute for Research in Dairying in 1938, and continued until the end of 1953.

The cheese-grader's assessments of 'body' or 'firmness', in particular, have been studied, since 'body' is a good indication of flavour and keeping quality. Furthermore, it can be assessed more readily than flavour and with less structural damage to the cheese. Cheese are usually graded after being kept in store for about a month from being made, the grader assessing 'body' by pressing his thumb into each cheese and, if he is doubtful about the quality, taking out a sample and tasting it. The cheese are then graded for quality and dealt with accordingly.

Graders are undoubtedly looking for quite large differences and attempting to assess whether each cheese is up to a certain standard, rather than trying to compare one with another. The grading has to be done to 'mental standards', since a cheese will change in quality continuously, and it is thus not possible to keep a 'standard' cheese for comparison purposes.

Since it now seems that skill at making relative judgments is not closely related to skill at making absolute judgments (Sheppard, 1954), it is perhaps unfortunate that so much research was devoted to studying relative judgments, although this research has proved interesting enough in itself (Harper, 1952; Scott Blair, 1949). More recently, absolute judgments of the kind made by the cheese grader have been studied by Harper and Baron (1950), Scott Blair (1954) and Sheppard (1952, 1953, 1954) and some of these studies have shown clearly that, in practical judging conditions, absolute judgments made from mental standards are very unreliable.

In all this research, however, no systematic attempt has been made to analyse the basis of sensory impressions on which the grader makes his judgments, although Katz (1938) and, later, Harper (1950) had pointed out the importance of this problem in an investigation of craftsmanship. The only investigations of this kind were made by McKennell (1951) and Sheppard (1953), both of which seemed to show that ability to apply a constant finger pressure was not necessarily related to skill at firmness discrimination, as Harper (1950) had suggested. Sheppard (1953) also showed that a strong subject is not necessarily a better judge than a weak one, even though he can deform the samples by a greater amount.

The judgment of firmness is, however, somewhat easier to analyse than most. Thus with flavour judgments it is rarely possible to do more than examine self-consistency and, even for judgments on lifted weights, for which it is possible to say that a judge is incorrect, it is not easy to tell if a mistaken judgment results from fluctuations in strength or from a faulty interpretation of sensory cues. With a firmness judgment, however, it is possible to examine the way in which a subject deforms a sample when judging it, and to relate this both to his judgment and to an instrumental measurement of firmness.

Two such experiments were therefore done, relating the amount of a finger and thumb movement to each absolute assessment for firmness and to corresponding instrumental measurements on the same deformable samples.

EXPERIMENT I: THE MOVEMENT OF THE THUMB IN RELATION TO THE FIRMNESS OF A CHEESE

Judges were asked to rate some cheese for firmness after thumbing each cheese four times at selected points. A seven-point 'graphic' rating scale was used for these ratings. By fastening a pin on the top of each judge's thumbnail as an indicator, it was possible to obtain a crude measure of the distance through which the thumb moved by observing the movement of this pin against a ruler, by means of a cathetometer telescope. By making the judges call out when they started and when they stopped applying pressure, it was also possible to take a crude measure of the total time for the thumb movement with a chronoscope.

Three expert cheese judges all rated the same seven cheese on one occasion and, in a later experiment, six judges, including two from the first experiment, all rated another seven cheese. Measurements were also taken for these cheese with the 'ball-compressor', an instrument which in mechanical action closely simulates the subjective judging procedure, and which gives readings which correlate highly with such subjective assessments (Baron and Harper, 1950).

Correlation coefficients were made between the following measurements: (a) ratings for firmness and amount of thumb movement; (b) ratings for firmness and time taken for a thumb movement; (c) ball-compressor readings and ratings for firmness; (d) ball-compressor readings and amount of thumb movement; and (e) ball-compressor readings and time taken for a thumb movement.

The resulting correlation coefficients are listed in Table 1. Since it was not expected that any one judge's ratings would be consistent from one experiment to another, or that ratings made by different judges would agree, the data for each test by each judge are considered separately. This means that with only five degrees of freedom most of these correlations are not significant, although where a number of correlations of one type have certain common features, tentative conclusions may be drawn.

TABLE I: THE MOVEMENT OF THUMB IN RELATION TO THE FIRMNESS OF A CHEESE

	r FOR RATINGS AGAINST AMOUNT OF THUMB MOVEMENT		*r* FOR RATINGS AGAINST TIME TAKEN FOR THUMB MOVEMENT		*r* FOR BALL-COMPRESSOR READINGS AGAINST RATINGS		*r* FOR BALL-COMPRESSOR READINGS AGAINST AMOUNT OF THUMB MOVEMENT		*r* FOR BALL-COMPRESSOR READINGS AGAINST TIME TAKEN FOR THUMB MOVEMENT	
	(a)		(b)		(c)		(d)		(e)	
TEST	I	II	I	II	I	II	I	II	I	II
Judge A ..	−0.02	+0.80	+0.49	−0.60	+0.98	+0.97	+0.09	+0.85	+0.49	−0.61
Judge B ..	+0.16	+0.89	−0.20	−0.21	+0.77	+0.72	+0.28	+0.80	−0.29	−0.82
Judge C ..	+0.54		−0.21		+0.86		+0.59		−0.14	
Judge D ..		+0.85		−0.25		+0.92		+0.92		−0.45
Judge E ..		+0.46		+0.40		+0.83		+0.34		+0.03
Judge F ..		+0.45		−0.31		+0.68		+0.03		−0.76

With one exception, the correlations are positive between the ratings for firmness and the amount of thumb movement, showing that larger thumb movements are associated with rating a cheese as 'soft'. This suggests that judges may, in fact, be applying a relatively constant load and judging firmness by the amount that the cheese deforms. On the other hand, judges A and B have significantly lower correlations in their first test as compared with their second, although their ratings correlate as highly with the ball-compressor readings in their first test as in their second. Thus, in their first test these judges cannot have been judging by relative differences in deformation and it may be that they were attempting to deform each cheese by the same amount and judging by the pressure required to do this. It is of interest that judge A commented that he always tried to apply a constant load and judged by differences in deformation, so it seems that his introspections must have been at fault for test 1.

Lower and in most cases negative correlations were found between the time taken for a thumb movement and ratings; thus not only are larger thumb movements associated with rating a cheese as 'soft', but also a longer time is taken to make this relatively larger deformation. It is not certain, from this evidence, that these judges rely more on differences in amount of deformation than on differences in time taken to compress samples, nor does it seem that the deformation of a sample would provide a more reliable cue, if these two subjective 'measures' of time and deformation are compared with the ball-compressor values.

It will be observed that the correlations between ball-compressor readings and ratings are not only high, but are, with one exception, higher than the corresponding correlations between ball-compressor readings and the amount of thumb movement. Since the relationships appear to be linear in both cases, it seems that judges may be influenced by sensory cues which cannot be observed 'externally', or that they can compensate for certain motor inaccuracies; thus, for example, a judge may be able to tell if he applies a smaller load than usual.

EXPERIMENT 2: THE MOVEMENT OF THE THUMB IN RELATION TO THE FIRMNESS OF SPRING SAMPLES

Since only crude measurements could be made in the previous experiments, some similar tests were carried out for which more accurate measurements were possible. Under practical grading conditions, each grade or rating value usually applies for a range of samples, and it may well not be possible to say what is the 'correct' grading for a given sample. In the tests now to be described subjects had to assess which one of a set of eight spring samples had been given to them. This procedure, although 'unreal', is convenient since it makes it possible to score judges as right or wrong and to estimate the magnitude of an error in terms of objective measurements. In this case an error would be defined in terms of the difference between the compression modulus of the sample given and the sample that it was guessed to be, so that, by averaging errors with and without 'sign', it was possible to find out the magnitude of a subject's average 'error' and examine any 'bias' to rate the samples generally as too soft or too firm.

Small helical spring samples 2.5 cm. in height and 2.0 cm. in diameter, fitted with plastic end caps, were presented in a finger dynamometer connected up to a chronoscope, so that the greatest amount by which a sample was compressed and the time taken to deform the sample could be recorded. The compression modulus of each sample was determined and the percentage differences in firmness (△ n) were calculated for each possible pair of samples, △ n being defined as [(deformation of softer cylinder under load — deformation of firmer cylinder) ÷ deformation of softer cylinder] × 100.

In each test twenty-five assessments were made by each subject, with a constant time interval of 14½ seconds between each. Each sample was compressed once only between finger and thumb. The samples were presented in a random order according to their firmness, seven samples being presented three times each and one sample, the same for each test, was presented four times. The subjects did not know that the samples were selected in this way, although they probably expected that each sample would be presented equally often over a number of tests. One hundred and sixty-six such tests were done in all by thirteen subjects, over a period of about a month. Most of these subjects had had considerable experience at making absolute judgments with such samples, but none were expert at cheese judging or had taken part in the earlier experiment.

Results:

A detailed analysis of all 166 tests was not possible. A careful examination was, however, made of all the tests done by six subjects and conclusions were otherwise drawn from a comparison of a few tests by each subject and from a study of averages from simpler scoring methods.

The results can best be described in three sections, the first dealing with the amount of time taken to deform a sample and the amount by which it is deformed, the second with an attempt to relate these to the absolute judgments and the third with a study of the judgments by themselves.

Section 1 : *The deformation of a sample and the time taken to compress it.*

Three subjects had done only one test each. These three tests, and three done by each of the other ten subjects, were examined by means of 'analysis of variance'.

From Table 2 it can be seen that for 30 tests out of 33 there were significant differences in deformation for different samples, but that it was rare for the differences in deformations between replicate samples, or for the differences in time of compression between samples or between replicates, to be significant. For one subject (Q), in two out of three tests, there were significant differences in deformation for replicate samples, the load applied to the samples decreasing during one test and increasing progressively in the other. For one subject (L) there were significant differences 'between samples' for the time taken to compress them in two out of three tests.

TABLE 2: RESULTS OF ANALYSIS OF VARIANCE ON THE DEFORMATIONS OF SAMPLES AND THE TIME TAKEN TO COMPRESS THEM, FOR 33 INDIVIDUAL TESTS MADE BY 13 SUBJECTS

PROBABILITY THAT A REAL DIFFERENCE EXISTS

	P<.20	P>.20	P>.05	P>.01	P>.001
Difference in deformation between samples	1	2	3	6	21
Difference in deformation between replicate samples	22	6	3	2	0
Difference in time of compression between samples	18	10	2	3	0
Difference in time of compression between replicate samples	19	11	3	0	0

When the averaged deformation for each sample for each of these 33 tests were compared by analysis of variance, it was found that the differences between tests and the differences between samples were both significant at $P > 0.001$ level.

Although there were no significant differences between samples for the time taken to compress them, for individual tests, graphs made for one test by each subject showed that there was generally some relationship between the times taken to compress a sample and its firmness. The amount of compression was more closely related to the 'objective' firmness of the samples than was the time for compression although, in both cases, the relationship was apparently linear.

TABLE 3: CORRELATIONS BETWEEN THE DEFORMATION OF EACH SAMPLE AND THE TIME TAKEN TO COMPRESS IT, AVERAGED FOR ALL TESTS BY EACH SUBJECT

SUBJECT	NUMBER OF TESTS FROM WHICH AVERAGES ARE TAKEN	CORRELATION BETWEEN DEFORMATION AND TIME OF COMPRESSION FOR DIFFERENT SAMPLES	SIGNIFICANCE LEVEL OF CORRELATION
K	15	+0.90	$P>0.01$
L	43	+0.93	$P>0.001$
M	15	+0.80	$P>0.02$
N	23	+0.72	$P>0.05$
O	20	+0.95	$P>0.001$
P	28	+0.93	$P>0.001$

More adequate evidence was obtained on this by relating the time taken to compress each sample and the deformation for each sample, averaged for all tests done by one subject. Data from those six subjects (K,L,M,N,O,P) who had done fifteen tests or more, were used for this purpose and the results of this analysis are shown in Table 3. In every case the correlations are significant and a graphical analysis shows that the relationship is linear in each case. It is worth noting that for those two subjects (L and M) who tended to deform samples progressively less throughout a test, the times for compressing these samples also became shorter, while for one other subject (K) who tended to deform samples more throughout tests, the times for compression also became longer. Even for these subjects there was only a 3 to 6% change in deformation, and the change in time taken was of the order of 2 to 9%. No such continuous trends were observed for the other three subjects.

The load exerted between finger and thumb, averaged for subjects K,L,M,N,O and P, was 17.2 lbs. For these subjects there were considerable differences in the loads applied to different samples, but there was no systematic relation between the magnitude of these loads and the firmness order of the samples.

Section 2: *Relating the judgments to the time taken to deform a sample and to the amount by which it is deformed*

Since it seemed likely that the deformation of a sample provided the most useful cue for judgment, correlations were computed between the following measures: (a) 'objective' firmness of sample given *vs.* 'objective' firmness of sample guessed; (b) 'objective' firmness of sample given *vs.* observed 'subjective' deformation of sample given; and (c) observed 'subjective' deformation of sample given *vs.* 'objective' firmness of sample guessed.

These were computed for 22 tests by 13 subjects and it was found that 12 of the (a) correlations, 21 of the (b) correlations and 11 of the (c) correlations were significant at the $P > 0.05$ level. The relationships appeared to be linear in all cases. Evidently the 'subjective' deformations are a better guide to the 'true' firmness of the sample than are the subjects' guesses. The differences in magnitude between the (a) and (b) correlations are generally large and the (b) correlations are greater for 20 of the 22 tests, although the differences would not all be significant.

This suggests that 'information' is lost between the squeezing of a sample and the subsequent guess at to its firmness, presumably resulting from a faulty interpretation of sensory cues and perhaps also as a result of some confusion of mental standards. This would not be surprising, but it is even more interesting that, whereas there is little or no relation between correlations (a) and (b), the correlations (b) and (c) for individual subjects are very closely related. In fact, there seems little doubt that the differences in judging skill are not attributable to differences in skill in applying a consistent load to all samples, but are a result of differences in skill at interpreting the sensory cues given by the resulting sample deformation or in relating these cues to mental standards. In fact, a difference in sensory or perceptual skill rather than a difference in motor ability.

Summing up, therefore, it may well be that subjects do judge more by the deformation of a sample, than by the time taken to compress it, while deformation certainly provides a more accurate cue to the firmness of a sample. Harper (1950) was probably right, therefore, in claiming that a judgment of firmness must depend on a subject's ability to deform samples consistently; and McKennell's (1951) and Sheppard's (1953) findings that differences in skill between subjects were not related to skill at applying consistent pressures 'outside' the test situation can now be explained. In fact, all subjects used in this experiment had very high (b) correlations, and they are similar for all subjects; thus all subjects are highly skilled at applying consistent pressures and there are not sufficient differences between subjects to be able to explain differences in judging skill in such terms.

It is true that subjects may not judge entirely by sample deformation; different judges may use different methods and the very analysis of the judgment process may destroy its essential features. Most judges, when questioned, claim that they judge by differences in deformation, but introspections may be misleading and it seems certain that judges A and B in the experiments on cheese must have used some cues other than deformation in their first experiment.

It is perhaps worth adding here that, although no systematic analysis of this type of firmness judgment has previously been made, there are numerous studies with lifted weights which are relevant. Thus it would seem that judges are not readily able to lift samples with a constant force, even when they are aiming at a constant maximum force (Morgan, 1917). Woodworth (1900) considers that the extent of a movement can be judged accurately and that it is not inferred from a judgment of the force and duration of a movement, but Muller and Schumann (1889) considered that time is the basis on which judgments are made for lifted weights. Fullerton and Cattell (1892), however, considered that force could be judged more accurately than time, although Morgan (1917) criticises the experiments on which this conclusion is based. As with the experiments described here, however, this earlier research only indicates which cues are most likely to be useful: it does not show which cues a subject will use when left to make a spontaneous judgment.

To conclude this section, it is worth noting that attempts were made to relate changes in the average deformation of samples, between tests, to the average error and average bias (or tendency to call the samples too firm or too soft) between tests. This was done for subjects K,M,N,O and P, but the only significant relation found ($r=0.53$, $P>0.02$) was between average error and average sample deformation for subject O. This subject had a smaller average error for those tests in which he deformed the samples most.

Section 3: *Studying the judgments*

As previously stated, this type of absolute judgment is in some respects different to those made by a cheese-grader, nevertheless many interesting points were disclosed by examining these judgments.

TABLE 4: THE BIAS AND AVERAGE ERROR AVERAGED FOR ALL TESTS BY EACH OF TEN SUBJECTS

SUBJECT	K	L	M	N	O	P	Q	R	S	T
Bias (Δ n) ...	+1.62	+2.29	+1.02	+1.91	+0.07	+0.45	+3.73	+1.03	+3.05	+2.48
Average error (Δ n)	7.62	7.54	7.44	6.69	6.85	7.10	8.08	7.46	9.38	8.11
Number of tests from which averages are taken	14	42	12	18	17	27	7	11	4	3

+ for a bias indicates that samples are judged as firmer than true values.

In Table 4 the bias and average error are shown for those ten subjects who had done three or more tests under comparable conditions. The figures given for each subject are averaged from all his tests, although certain tests which called for slightly different judging conditions, which could be included in Table 3, were omitted from this table. There was no significant tendency for those subjects who had done most tests to have the lowest average bias or error. It was found that all subjects tend to judge the samples as too firm, so that, even after long experience with the samples, the judgments do not completely conform to the stimulus range, as would be expected from the work of Hollingworth (1910) and Tresselt (1948). It is of interest to compare these data with those discussed in the earlier paper by Scott Blair (1954), in which one subject (Subject L in this experiment) had an average error of 7.79 $\triangle n$, and an average bias of + 1.93 $\triangle n$ for 32 tests. It seems that this subject was able to make judgments just as successfully in these tests when the samples were presented in a finger dynamometer, as under less rigidly controlled conditions. In these tests the average error was even somewhat less and the slight though insignificant increase in positive bias may have been a result of friction in the dynamometer. An increased 'time-error' (Wever and Zener, 1928) is unlikely since the rate of presentation of the samples was similar for both experiments.

Examining the results given by these ten judges to see if the average bias or average error showed any progressive trends throughout a series of tests, it was found that only for one judge (L) was there a significant trend at the $P > 0.05$ level. For this judge it was the bias which tended to become more positive with time, although there were no progressive changes in strength which could account for this.

A somewhat more detailed study of the judgments given by the five judges K,L,M,O and P showed that all except judge K had a smaller average error in the second half of a test, although the differences were not significant for any judge. Only for one judge (L) was there any significant change in bias, and the bias for this judge again tended to become more positive with time. The results of this examination of the data are shown in Table 5; the data given by Judge N were not analysed since this judge would sometimes alter earlier responses after having judged a number of samples.

TABLE 5: CHANGES IN THE AVERAGE BIAS AND AVERAGE ERROR BETWEEN THE FIRST AND SECOND HALVES OF A TEST, AVERAGED FOR ALL TESTS BY EACH SUBJECT

SUBJECT	NUMBER OF TESTS FROM WHICH AVERAGES ARE TAKEN	BIAS (Δ n) FIRST HALF OF TEST	BIAS (Δ n) SECOND HALF OF TEST	SIGNIFICANCE OF DIFFERENCE	AVERAGE ERROR (Δ n) FIRST HALF OF TEST	AVERAGE ERROR (Δ n) SECOND HALF OF TEST	SIGNIFICANCE OF DIFFERENCE
K	14	+1.33	+1.90	$P<0.10$	7.50	8.53	$P<0.10$
L	42	+1.63	+3.05	$P<0.10$	7.55	7.52	$P<0.10$
M	12	+1.37	+0.66	$P<0.10$	7.93	6.95	$P<0.10$
O	17	—0.10	+0.26	$P<0.10$	7.24	6.46	$P<0.10$
P	27	+0.28	+0.62	$P<0.10$	7.36	6.83	$P<0.10$

+for a bias indicates that samples are judged as firmer than true values.

Correlation coefficients were computed between subjects' average errors and average biases for the first three samples in each test, against average errors and average biases for all twenty-five samples. This was done for five judges and there was found to be a highly significant relation between the average errors obtained in these two ways for judges L and P ($P > 0.001$)

and for judges K and M (P> 0.01), but that there was no significant relationship for judge O (P< 0.10). There was a significant relationship between the bias on the first three samples and the average bias for all twenty-five samples for subject K (P> 0.001), but for all other subjects the significance of the relationship was less than 0.10. This suggests that, in general, if subjects make large errors at the outset of a test, they will continue to make large errors throughout the test, but that it would not usually be possible to predict a subject's bias throughout a test from his bias at the beginning of a test.

Scott Blair (1954) had shown for one subject that when the identity of a sample was guessed correctly, the probability that an immediately following guess would be correct was improved, but that a smaller error than usual was not found unless the subsequent guess was completely correct. This point was not examined with these tests, but it was possible to show that when a subject had a large number of judgments completely correct, he had a smaller average error than usual for all other judgments in that test. This was true for all five subjects, but only significantly so with subjects K, L and M.

There was no tendency to avoid giving the same guess twice, when the same sample was, in fact, presented twice running. Thus for the 206 occasions on which the same sample was presented twice running to these five subjects, on 26 occasions they would have been guessed to be the same by chance and 33 such guesses were in fact made. This number of guesses is not significantly greater than chance.

A more detailed examination of these 206 guesses showed that marked contrast effects existed within such pairs of samples, although these just failed to be significant at the 0.05 level. The effect was such that if a firm sample was presented twice in succession, it was judged to be softer on the second presentation than on the first. The opposite effect was found when a soft sample was presented twice.

It is of interest, therefore, that when all the correct judgments made by these five subjects were examined, there was no evidence that any individual sample had a significantly better chance of being judged correctly if it happened to be presented immediately after one particular sample rather than another.

The frequency with which guesses were made for each sample proved of interest (Scott Blair, 1954). As stated previously, seven samples were presented three times each and one sample (H) four times in each test, so that something like a rectangular distribution of guesses would have been expected. The distributions of guesses for six subjects (subject N's being relevant for this purpose) are shown in Figure 1.

It can be seen that only for subject O is the distribution similar to what would be expected; that subjects L, M and P have distributions skewed towards the soft end of the scale; that subject N's distribution is approximately 'normal' and that subject K's distribution appears to be bimodal. The distribution of guesses for judge O is not significantly different to the expected

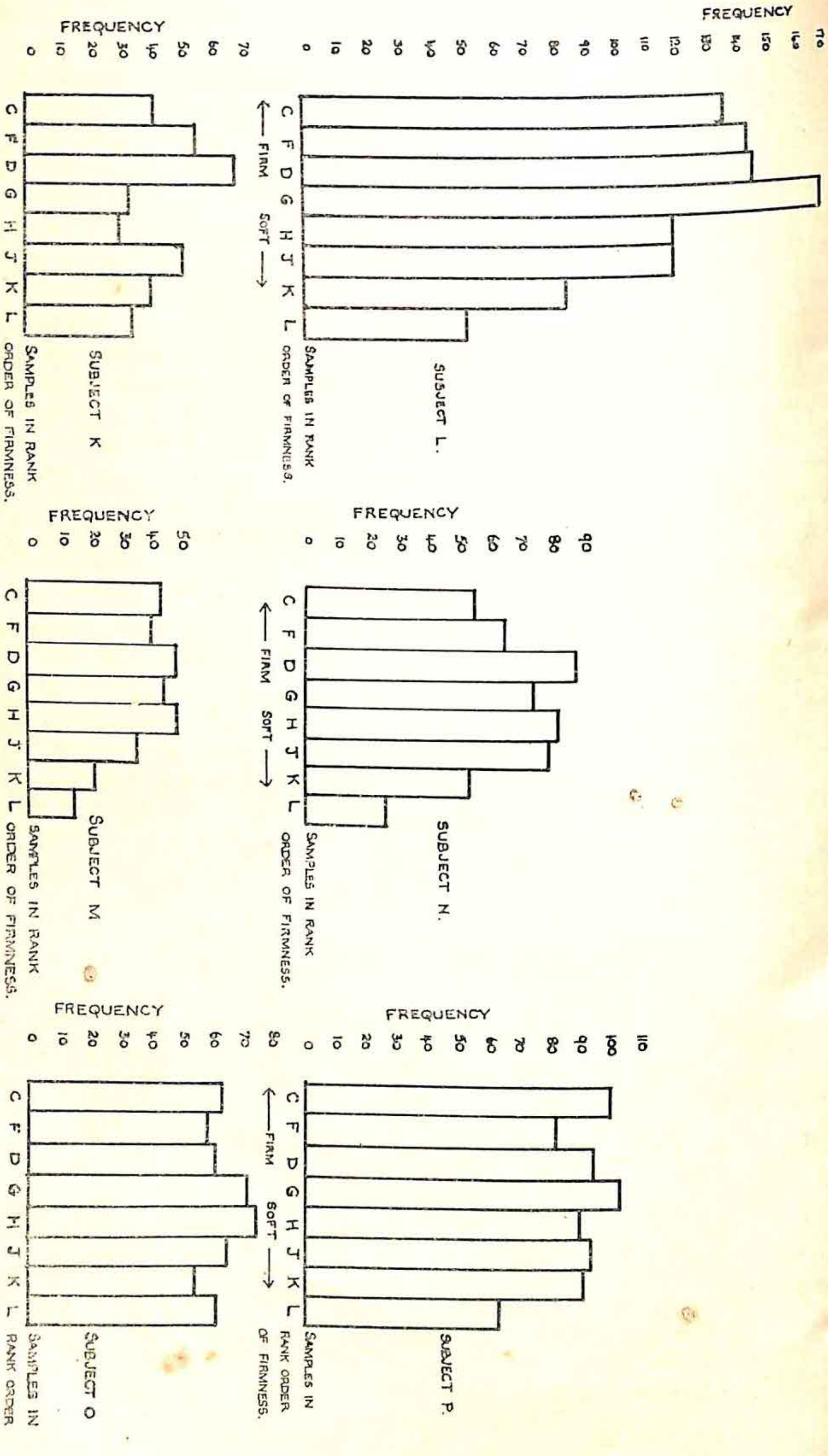

FIGURE I. HISTOGRAMS TO SHOW FREQUENCY WITH WHICH 'GUESSES' WERE MADE FOR EACH SAMPLE, FOR ALL TESTS DONE BY EACH OF THE SIX SUBJECTS

distribution ($\chi^2 P > 0.90$), for judge P the distribution just fails to be significantly different at the $P > 0.05$ level and for judges K, L, M and N the distributions are significantly different at the $P > 0.001$ level.

Examining the data from subjects K, L, M, O and P it was found that for four subjects (L, M, O and P) the distribution of successful guesses was significantly different to that which would be expected were they able to recognise each sample equally successfully. This was mainly because there were a significantly greater number of successful guesses for samples 'C' and 'L', the two extreme samples of the series: 143 successful guesses in all were made for sample 'C' and 98 for sample 'L', by these five subjects, as compared with an average of 61 successful judgments for the other six samples.

It was also found that for no one of these five subjects were the number of correct guesses proportional to the number of guesses made for each sample ($\chi^2 P > 0.05$ in each case). Samples 'C' and 'L' were again guessed successfully by a significantly higher proportion of the number of guesses for these samples ($\chi^2 P > 0.001$), than were the other six samples, when the data for all five subjects are considered together.

It is also evident that there is a close relationship between (a) the similarity of each subject's distribution of guesses to the distribution of samples given and (b) the average error for each subject. It may well be that it is the extent to which subjects 'prefer' giving certain judgments rather than others that is, in fact, the key to differences between judges in skill at making this kind of absolute assessment, which has already been suggested to be due to differences in skill at sensory interpretation, rather than to differences in muscular skill.

CONCLUSIONS

Taking the evidence from the two experiments together, there seems little doubt that judges generally estimate firmness by trying to apply a constant load and judging either by differences in deformation or by differences in the time taken to compress a sample.

It seems that differences in deformation give a more accurate cue for judging firmness, but it does not necessarily follow that judgments are based mainly on this, although introspections suggest that they are. In the experiments on cheese the ratings are more highly correlated with 'objective' measurements of firmness than is the amount of 'subjective' deformation, suggesting that in this case judges are influenced by certain other cues. In the more accurately controlled experiment with springs, the ratings are a less accurate measure of firmness than is the amount of 'subjective' deformation.

The finding that differences between judges result from differences in ability to interpret sensory cues rather than from differences in skill at applying a constant finger pressure, probably applies as much for judgments on cheese as for judgments on spring samples. The suggestion that this 'faulty interpretation' is primarily a consequence of the less efficient judge having marked preferences for certain sample numbers is only likely to apply for the rather special kind of absolute judgment made on the spring samples.

It must be remembered, however, that the evidence from both experiments is sketchy and that only crude measurements of finger movements were available for the experiments on cheese. It is also possible that the very analysis of such a judgment process may destroy its essential features.

SUMMARY

Studies of the cheese-grader's assessments of firmness are reviewed and a more detailed examination of the sensory basis on which absolute judgments for firmness are made is discussed.

There seems to be evidence that the differences in skill at this task, between judges, can be attributed to a faulty interpretation of sensory cues rather than to differences in skill at handling deformable materials. This is probably true both for cheese samples and for other materials which have physically 'simpler' properties. In the latter case the fact that certain judges had marked preferences for certain sample code letters seems to account for their inefficiency at making absolute judgments.

ACKNOWLEDGMENTS

The writer wishes to acknowledge the advice and help given him by members of the Department of Dairying at Reading University and the help given by others who took part in the experiments. The writer also wishes to thank Dr G. W. Scott Blair and Professor R. C. Oldfield for helpful advice; Miss S. D. Carrinci not only for helpful suggestions but also for administering the tests and for analysing the data; Mrs B. Murray for helping with the analysis of the data and the Agricultural Research Council for a grant for the period during which the research was carried out.

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Scientific and Professional Problems of the Psychologist in Industry

By ALASTAIR HERON

Dr Heron has for six years been a member of the Medical Research Council's Unit for Research in Occupational Adaptation. This paper was presented at a meeting of the Industrial Section of the British Psychological Society in London, October 12th, 1954, shortly after Dr Heron's return from a three-month visit to North America.

INTRODUCTION

THE main purpose of this paper is to direct fresh attention to matters of importance last raised in 1942 and 1949 in the pages of this Journal. Thirteen years (or even six) is a long time in the short history of psychology as a branch of science, and seems even longer when the activities of psychologists in industry are being considered. More important however is the very real danger that a new group of psychologists arising from the recent period of expansion may find themselves active in industry but possibly unalerted to the relevant scientific and professional problems last reviewed publicly before their time. It may also be appropriate to consider these problems afresh at a time when the International Association of Psychotechnics (Applied Psychology) is holding its twelfth congress in London, the first occasion on which British psychologists have had the privilege of acting as hosts.

What were these matters of importance, these scientific and professional problems? In brief, they can be subsumed under the general headings provided by the titles of Professor Hearnshaw's (1942) paper, "The Unity of Industrial Psychology", and of the 1949 symposium, "What is Industrial Psychology ?" (Hearnshaw, Knight, Rodger, Bartlett). A more precise answer may be found in another title, "Psychology as a Science, as a Technology, and as a Profession" (Bingham, 1953). The present paper is frankly devoted more particularly to a consideration of the need for more and sustained attention to the scientific side of our endeavours if the success of the technological and professional aspects is to be assured.

PSYCHOLOGISTS AND INDUSTRY

The connections of psychologists with industry can be roughly graded as 'interested', 'actively associated', and 'inside'. The first category may be said to consist, in the U.S.A. and Great Britain, of the 514 Fellows and Associates of the American Psychological Association who have joined its Division 14 (Industrial and Business Psychology) and the 160 Fellows and Associates of the British Psychological Society who have joined its Industrial Section. The 155 Diplomates in Industrial Psychology who have met the requirements of the American Board of Examiners in Professional Psychology (ABEPP) probably represent the solid core of the U.S.A. contribution to the "actively associated" sub-category; no comparable figures are available for Great

Britain, as no such professional examination exists there as yet. The number of American psychologists actually "inside industry" has been estimated by Bills (1953) at about 100, the figure for Great Britain being well below 20 even if generously wide interpretation is accorded to both the terms 'industry' and 'psychologist'. Comparable data for Continental countries and for other parts of the world are not available to me, but readers can no doubt provide the missing information about their own countries from their private knowledge of the prevailing situation.

These data are striking for two reasons: firstly, because they are only adequate enough to permit intelligent guesswork about the situation; secondly, because the numbers of those "inside industry" are so small. The inadequacy of our data for the purpose may suggest that we have not so far considered it administratively interesting or important enough to obtain more precise information about the nature and extent of psychologists' connections with industry. The relatively large pool of those 'interested' makes one wonder why they are interested, how they implement their interest, and—above all —what sort of psychologists they are. Can one assume that most of them regard psychology as a branch of science which they would like to see developed so that its findings may be applied in industry; or do many think of industrial psychology as itself a specialised branch of science in its own right ; or are the majority far less 'austere' (in their own judgment), thinking of industry largely as an " interesting sphere of human activity "? The safest answer is probably that all these emphases are well represented ; but it would be illuminating and perhaps useful to know the relative proportions. One thing seems certain: we cannot be *sure* that the last group is outnumbered by the two others combined ; yet such an assumption is all too frequently met. What is true of the 'interested' majority may well be also true, *pari passu*, of the small group working 'inside' industry ; but here an additional consideration arises in that such psychologists are employed, for the most part, neither as scientists nor as dilettante observers but as practitioners of a complex and variously-defined skill.

Such considerations undoubtedly influenced Bingham (1953) in preparing his thoughtful and thought-provoking paper already referred to above. He saw " the aim of psychology as science (as) to systematise and to generalise about facts of behaviour " observed under adequately controlled conditions. Applied psychology (or psychotechnology) he defined as " psychology in the service of aims other than its own", and was then able to regard the practitioner as " one whose occupation it is to utilise the techniques of the psychotechnologist for the benefit of his clients". So far, so good—and had Bingham stopped there, no ripple need have stirred the complacent calm of the psychological pond. He did not stop, however, and went on to add, " *Of these roles, that of psychological scientist is primary*". (My italics). This accords well with Hearnshaw (1942): " The progress of industrial psychology has made it more and more obvious that effective changes in the human side of industry can be brought about *only on the basis of a sound theoretical industrial psychology* . . ." (my italics).

The plain fact is that most psychologists connected in any way with industry will subscribe to both the statements quoted while displaying little or no concern about the small extent to which they are put into practice as guiding principles. Without minimising or ignoring the very real obstacles to such a consistent practice, I believe that only as we work our way past such obstacles will we obtain the respect necessary for their eventual removal.

SCIENTIFIC PROBLEMS OF THE PSYCHOLOGIST IN INDUSTRY

Obviously any selection here is to some extent a matter of personal interest insofar as one attempts to be specific. Perhaps it would be more useful to consider general headings and then to choose specific areas of enquiry as illustrations.

The first general heading may be described as "the discovery of new variables". This phrase comes from a paper by Cantril *et al.* (1949) in which the authors go on to say, "Scientific progress results from the ability to pick out the most relevant conditional relationships for empirical investigation—not by further analysis of established variables alone". A second and related heading, "the re-formulation of problems", is also stressed by these writers in the following terms: "It is often forgotten that the value of an experiment is directly proportional to the degree to which it aids the investigator in formulating better problems . . .", and this is reinforced by Flanagan (1952) when he says, "The terms in which an applied problem is formulated and the general research methods used are the determining factors in whether a research study yields a specific result or one which may be generalised". He here touches also on what might well serve as my third general heading, which can be called "the revival of attention to established principles in the relationship between theory and *ad hoc* problems".

(*a*) For illustrative material we can start with the fresh emphasis revealed in Ghiselli's (1951) evaluation of R. L. Thorndike's (1949) "differential prediction approach" to personnel classification. Ghiselli regards this as an example of "an attempt to formulate basic concepts and to state problems. The objective is to assign all individuals so as to achieve maximal overall effectiveness of the organisation. It is apparent that under such circumstances only limited use can be made of the individual's absolute level of ability, and the initial factor becomes the *differences* in level of ability for the various jobs". Here we have the beginning of a theoretical basis for the transition from "selection-by-rejection" to "selection-by-placement"—the integration of personnel selection with vocational guidance *inside* the undertaking.

(*b*) In the same article Ghiselli draws attention to the fact that "the psychologist has been content to accept the job as it stands" and suggests that in the light of their work on employee satisfaction Likert and Katz and their co-workers (Katz, 1949) "challenge management to explore individual needs further and to discover ways of changing jobs in these terms rather than just in terms of methods of work, duties, and the like". Surely the challenge is also, and perhaps primarily, to the scientist-psychologist interested in, associated with, or employed by, industry? This is a re-formulation of the 'job-demands' problem big enough to make it a new variable, but progress will depend on close attention to established principles in the relationship between theory and *ad hoc* problems in the actual situations concerned. It thus serves to illustrate all three of our main headings.

(*c*) Turning now from job demands to another aspect of selection, worker evaluation, Sartain (1951) has asked some questions (quoted below) which give point to a neglected sentence in Ghiselli and Brown (1948): "If an improvement in the accuracy of foremen's ratings of workers is desired, it should prove more fruitful to discover foremen's attitudes towards the rating of employees than to devise a better rating scale." Sartain reviews the current variables of worker evaluation technique, and goes on to say, "But these are not the fundamental questions of worker evaluation. They have little importance indeed alongside questions like these:

(i) What is the effect on motivation of workers when told they are to be evaluated by their bosses?

(ii) Is efficiency rating the best, or even a satisfactory, method?

(iii) How well prepared is the average supervisor to explain his ratings in a way that will be genuinely helpful? What approaches should be used?

(iv) Do supervisory comments usually point out realistic ways in which workers can improve? *Do* workers improve as a result?"

(v) How do the supervisors who do the ratings feel about them?

(vi) What is the effect on the raters of the 'tricks' we use (e.g., Forced choice) to get 'better rating'?

(vii) Is it possible that almost any scheme of rating will get successful results when there is mutual trust and confidence between workers and their bosses, and that literally any scheme will fail when such mutual respect is lacking?"

We not only don't know the answers to most of these questions, but we (and I must include myself) have failed to be sufficiently concerned as scientists about our ignorance even when given opportunities to remedy it.

(*d*) Criteria of occupational adjustment are at last receiving more rigorous attention. Apart from a number of recent careful studies, it is noteworthy that the Division of Industrial and Business Psychology devoted a symposium to criteria at the 1954 Annual Meeting of the A.P.A. in New York, and that the Twelfth Congress of Psychotechnics in London has included "the problem of criteria" in a plenary session. Encouraging as this belated attention may be, however, it is unlikely to prove fruitful unless this vital subject becomes the target for new theoretical consideration and genuine experimentation.

(*e*) During the last 10-15 years steadily increasing attention has been given to problems of motivation in industry, but it can hardly be denied that this attention has been disproportionately technological in Bingham's definition. There is a pressing need for studies in this area which meet the challenges of Bingham, Hearnshaw and Cantril. Happily, one of the more recent in the rapid succession of American books on industrial psychology has recognised this, and Chapter 15 of this book by Ryan and Smith (1954) entitled "Industrial Motivation: Specific Hypothesis", deserves close attention and early implementation.

These then are representative of some of the scientific problems awaiting immediate attention by psychologists interested as scientists in the field of industry, together with illustrative indications of new (or renewed) thinking of an appropriate kind. At this point the psychologist 'inside' industry, or "working for it", may ask somewhat testily what all the fuss is about. Surely, he may be heard saying, we have enough to do getting facilities to solve immediately pressing problems without adding to our troubles through promulgation of this idealistic stuff about the need for a "scientific approach"? With this objection in mind, I turn now to consider the present status of industrial psychology.

PROFESSIONAL PROBLEMS OF ACCEPTANCE AND PRESTIGE

Consideration of the history of psychology in the occupational field during the first quarter of this century seems to justify an optimistic prediction for the next quarter century. Such optimism is, however, not wholly justified in the event. For this statement there are two pieces of evidence: the number of psychologists employed *as psychologists* by industry; and the reduced tempo of scientific advance in the areas of psychology most closely associated with occupational phenomena. It may be profitable to consider some of the probable causes, remembering as we do so that there is almost certainly a complementary relationship between the two facts just adduced.

(*a*) Gorsuch (1952) asked the following questions:

(i) Why do psychologists have such difficulty in gaining acceptance in industry?

(ii) How do psychologists spend their time in industry?

(iii) What can we do to provide better psychologists for industry?

(iv) How well are psychologists equipped to solve business problems? Are we getting too smug? Do we have the necessary tools? If not, what are we doing about it?

What caused him to ask them?

(*b*) Anderson, Works Manager in one of the large plants of General Motors, in a symposium edited by Lawshe (1951) stated that "the reason we don't use more psychologists in industry is that we still have to convince top management that the industrial psychologist has a real contribution to make".

Is this the opinion widely held in industry?

(*c*) In his contribution to the symposium in this Journal, Rodger (1949) came to the reluctant conclusion that "the slowness of the progress of industrial psychology as a science has been produced only partly by adverse circumstances: we must recognise the embarrassing fact that a number of industrial psychologists display neither a truly scientific outlook nor a firm grasp of the subject matter of their particular science".

What do we have to learn from this?

(*d*) An anecdote from British industry may serve well to complete the picture. The co-operation of a particular second-line executive was essential to the integrity of a piece of psychological research for which general first-line approval had been obtained. This man had "done what was necessary" previously, but without enthusiasm and in an atmosphere of formality. The investigator explained the rather onerous requirements of the next stage, made it clear that he realised how much inconvenience it would cause and apologised for the necessity, but added that if it could not be done properly he would prefer not to do it at all. At this point, the attitude of the executive altered in a way that could only be described as 'dramatic', and with warm friendliness he said, "This is the *real* thing". From that point on, he spared no effort to ensure the scientific success of the project.

Let it be made clear at once that the relatively low prestige and guarded acceptance of psychology as a branch of science in the occupational field are by no means solely due to an accumulation of individual and institutional failure. The difficulties of psychology itself are well-known enough to require no further elaboration, but they must not be overlooked. Fear and suspicion, often irrational in origin, have contributed their obstructive share; lack of financial support has, until very recently, been a well-nigh insurmountable obstacle. These things are just as true and just as relevant as the facts considered above, but as psychologists we should look to their causes. Perhaps, as was said in the Lawshe (1951) symposium by Folley of the Ford Motor Company (U.S.A.) "industrial psychologists have not been jealous enough of their calling. It took the engineering profession years to win the confidence of production men in industry . . . if a given industry *does* expect an overnight miracle, then either the management is unreasonable or inept, or pseudo industrial psychologists have led managements to believe that . . . they have the ability to reach solutions in human problems with a mental slide rule". Neither modesty nor common-sense permit us to assume that the problem is *always* the ineptitude of the management.

We must establish respect for psychology in the only manner possible—the long hard way of rigorously scientific practice. This is not, however, to be identified with the narrowly academic, with a policy of avoiding defilement. The way to establish respect is to go out into industry and investigate problems in which industry is interested, but within a thoroughly rigorous theoretical frame of reference that is capable of preserving the unity of industrial psychology—that can prevent that fatal separation of science, technology and 'professional' practice which has so often characterised the events of the past thirty years. As Bills (1953) puts it, "It's a very thin and sometimes wavy line between the fundamental concepts that form at least part of our contribution to management, and our acceptance of the less rigid principles of proof that prevail in the management field."

THE PROFESSIONAL PROBLEM OF SCIENTIFIC RESPONSIBILITY

What are the positive steps which newcomers to occupational psychology can reasonably be asked to take? How can their efforts to do so be supported academically and professionally?

We must try to ensure that all those who seem likely to do psychological work in industry are thoroughly prepared (a) for what they are likely to find there, and (b) for what sort of challenge any such opportunity must present to them as scientists and as persons. I suggest that if such preparation is to be the rule rather than the exception, we must at last begin to apply our own concepts to the study of this problem. There is clearly a need for close study of the motivation and training requirements of a job involving the dual role of scientist and practitioner. With some adequate knowledge of these requirements, it would be easier to ensure meaningful vocational guidance for our students and graduates and to shape our undergraduate and post-graduate training methods.

There seems to be a place for more symposia, workshops and seminars where the problems of industrial psychology can be re-examined by psychologists and students broadly representative of the three major functions—science, technology and profession—which have been identified by Bingham and others. As these occasions become more frequent, and as we learn how best to conduct them, it is not idealistic to hope that academics would become rather less "intolerable in their attitudes" than they have recently been found by one distinguished commentator, and also that those "inside industry" would become less defensive and more able emotionally to perceive the benefits that would accrue from faithful practice of their primary role as scientist.

If these provisions are to be made, there is a task for our professional and learned societies. Too many standing committees are often a millstone round the neck of an organisation, but we all know that much good work can be done by an *ad hoc* committee, with definite terms of reference, a strictly limited life, and not too many members. The task of such committees would be to provide an adequate stimulus to potential pioneers, a note of liberation for those who are enchained by dull repetition of half-truths which purport to explain why little can be done to raise the status of occupational psychology.

There is one other way in which we can face the professional problem of scientific responsibility in the occupational field. This way, though not new, has received its most adequate recent exposition in the address of J. McVicker Hunt (1952) as President of the American Psychological Association, from which address a brief quotation is apposite:

"The goal of this form of enquiring enterprise (science) is dependable knowledge, warranted assertions or conceptual systems . . . (this) requires then, above all else, a close and proper conjoining of observing and doing . . . with generalised thinking. . . . Our compact with society would be to give free services—and, let me add, good services—in exchange for activities on the part of clients which are not immediately relevant to their goals of getting help. We shall not be the first combined science and profession to do this."

I believe that the time is ripe for fresh exploration in this direction. It seems a realistic and responsible approach.

CONCLUSION

Let me summarise what I have tried to say. Those most closely connected with occupational psychology are far from happy, either about its scientific progress or its public status. There are scientific problems which can engage our immediate attention with hope of real profit, provided their solution is sought in the place where the problems arise. Facilities to do this work are not easy to obtain, but increased respect for psychologists as scientists would unlock doors in a way which expediency and compromise have failed to do. We can and must select and prepare students of psychology in such a way that they can fulfil technological and professional roles from the point of view of men and women who are primarily scientists. Our professional organisations can assist this movement by good committee work and by stimulating their members to organise suitable occasions for the thorough, honest and *continuing* examination of the relevant problems. Fresh attention should be given to ways in which psychological services of high quality may be exchanged for active assistance by industry in the solution of basic theoretical problems in which they are at present unable to see immediate relevance.

This paper has not sought either to allocate blame or to provide excuses for failure. No one need become defensive when all are looking forward to ways of doing better and to enjoying the adventure of change. The unity of industrial psychology quite clearly cannot be legislated into existence; it depends more on our individual integrity than we are usually inclined to believe.

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Timing

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SUMMARY

In many skills response needs to be made within finite limits of time. These time limits are ultimately determined by the frequency of successive signals for action.

Reaction time theory has limited value for an understanding of skilled performance because it is largely based on the assumptions that signals will occur suddenly and will require immediate response. Generally, however, signals can be detected in advance; frequently the problem will be not the hastening of response, but the delaying of it until the best moment. Timing permits the skilled operator to manipulate the time within the inherent limits. Good timing in fact provides the optimum temporal conditions for response.

INTRODUCTION

In his paper on skill, Bartlett (1943) drew attention to the fundamental importance in skill of *timing*. In regarding the principles of skill as it is commonly understood as also underlying biological adaptation, and by insisting that timing provided the key to those principles, Bartlett re-opened the problem of man's relationship with his temporal as well as spatial environment. It may once have been feasible to consider time as an experience; much experiment has failed to illuminate this concept. But timing describes a feature of adaptive *behaviour* familiar to everyone. Once the idea of time as an analogue of space is abandoned, it becomes possible to construct some kind of picture of the way in which timing affects behaviour, and to realise that the experimental emphasis needs to be on change as much as on duration.

Skill is not an act but a process. In almost all the skills of everyday life, part of the process will involve dealing with an ever changing stimulus field made up of many constituents. Different sections of this "display" compete for the attention of the operator, who, with his growing acquisition of skill, will be increasingly adept at distributing his attention so as to be closely in touch with the changes that occur. Some of these changes or signals will have an inherent attention getting character, such as warning lights or motor horns. The majority will not, and part of the operator's skill will be his ability to know what to attend to, not because a critical signal has occurred, but because he knows that one is imminent.

Whether the display is entirely synthetic such as an instrument panel, or partly natural such as traffic, what is almost certain to be of major importance, is the amount of time available between critical signals, and the amount of ground that must be attentionally covered in that time. This does not necessarily mean that the skilled operator rapidly and continuously scans every possible source of signals. There will rarely be time for that. But what he probably must do continuously is to decide in advance which signal source will provide the next signal.

TIME AS AN EXPERIMENTAL VARIABLE

If the environment is one which has been entirely designed, it is quite possible that signals will occur at equal intervals of time. The operator has the great advantage of knowing when to expect a signal. If signals are initiated by more than one source there is at once the possibility of two concurrent regular but different series of signals. In this case the intervals between successive signals need not be equal, and the operator may have to judge when the next signal will occur. If the number of sources each providing periodic signals is sufficiently large, but with the periods differing by small amounts from one source to another, then the intervals between successive signals will approximate to what would occur if the signals were random in time (Cox & Smith, 1953). It has been shown (Conrad, 1954b) that even with four such signal sources the distribution of the interval between signals resembles the exponential.

If the environment is not entirely man-made (and often when it is), the skilled performer will very largely have to deal with signals which are apparently occurring at random with respect to time. In relation to the mean rate at which signals occur, there will be far more short intervals between signals than long.

Consider merely the frequency per minute with which successive signals will occur within 0.25 sec. of each other. It the signals are presented at equal intervals, no signal will ever occur within 0.25 sec. of the previous until the speed is 240 signals per minute. At this speed, every signal will be 0.25 sec. from the previous one. If, however, signals are presented at random intervals, when the mean speed is only 40 signals per minute, 6 of them will be within 0.25 sec. of the previous. When the mean speed is 120 signals per minute 47 of them will occur within 0.25 sec. of the previous. It is evident, therefore, that in theoretical studies of skill, the temporal structure of the display is as important as the mean rate at which the signals are presented. If at any point in the stimulus-response link there is an absolute time threshold, then the effect of this threshold on overall performance may be very different when the same number of signals occurs at equal or random intervals.

When signals occur at equal time intervals the time-limits available for every response are inherently equal. The actual limits may be affected by advanced or retarded responses, but the display itself exerts no influence. On the contrary it assists performance by assuring the subject of a given temporal framework for an infinite time ahead. When the intervals between signals approximate to random, the time available for any response depends not only on what the subject does, but also on the temporal structure of the display. If signal-3 is close to signal-2, and signals require response in the order in which they occur, then the time limit for response-2 is curtailed at the end. This is a limiting factor imposed by the display. But if the subject's response-1 was itself late, then response-2 is also limited at the beginning, not so much by the display, but rather by the subject's treatment of it. Thus time-limits may result from an interaction of temporal display characteristics with recent responses to the display. The skilled subject makes things easy for himself.

In previous papers some experimental results were presented which demonstrated certain ways in which subjects appeared to adapt in this way to time-limits inherent in a continuously changing display (Conrad, 1954a, 1954b). Basically the task presented a series of signals at irregular intervals, the response to each of which (tapping a morse key) was recorded. All signals were represented by the coincidence of a continuously visible pointer and continuously visible marks on a dial face. But there were four dials with the pointer on each revolving at slightly different speeds. The problem for the subject was not to respond as soon as possible after the signal had occurred, but to anticipate on which dial the next signal would occur before it occurred, and then respond at the moment of coincidence. The subject's skill lay in making up his mind what to do next before the resulting action would be too late. Because of the temporal arrangement of signals mentioned, the intervals between them varied considerably irrespective of the average rate at which signals occurred. But this average rate, the signal speed, was experimentally varied and this too was shown to have an effect on performance independently of the amount of time between any two successive signals. Within the framework of this task some of the factors can be discussed which appear to determine the effectiveness with which the time-limits were dealt with.

REACTION TIME THEORY IN THE STUDY OF SKILL

The primary question is why, even at very slow signal presentation rates, were responses not more accurate? Theoretical perfection in skill is of course a fruitless notion. There are always limits within which the response is satisfactory, and the extent of the limits varies with the skill. In this task response was demanded at a moment which was easily recognised visually, and it was shown in the papers cited that the scatter of the responses around this point could not be attributed to chance, nor to random errors of aiming. Nor would the data sustain a theory that subjects subjectively accepted the whole width of the target mark as satisfactory. No concept of a fixed spatial standard bears examination.

The simplest theoretical framework into which the results might have been fitted, and which superficially appears to be most possible would have been traditional reaction-time theory. Fundamentally a series of responses were made to a series of signals. In particular the discrete nature of individual signals and responses strongly suggest the applicability of recent discussions (Hick, 1952; Welford, 1952) on reaction times. It is evident that the concept of load (number of independent streams of signals) (Conrad, 1951) is closely related to that of choice in the classical reaction-time experiment. At the same time, the concept of speed used here (mean number of signals per unit time) resembles in many ways the variable used by Telford (1931), Craik (1948) and Vince (1948) who varied the interval between successive stimuli in reaction-time experiments.

There were, however, certain fundamental differences in the character of the presented signal series which would render comparison with the studies cited less relevant. This difference, in turn, suggests certain limitations of reaction-time theory as it now stands, in relation to studies of skill. In the case of the choice reaction-time experiments, although there is the common element of choice, no worker appears to have considered the situation where the interval between successive signals changed—a feature of skills which is extremely common. Secondly, in the case both of choice and simple reaction-times, the experimental situation was almost invariably such that the subject had to deal with a signal which appeared *suddenly*. Obviously in all cases the subject knew that a signal was going to occur some time, but no direct *sensory* information was provided to indicate when it would occur. Indeed if this were given, there would be no reaction-time to measure (Poulton, 1952). It is this anticipatory quality of the signal series which characterised the studies under discussion.

In the experiments reported by the author, the subject was provided with all the information necessary to determine when the signal would occur. When the speed was varied, the average time available to interpret the information changed, and, for any given speed, the time available to interpret the information relevant to the next signal varied from one signal to another over a wide range. But the duration of this time, at least relatively, was also given in the display.

In reaction time experiments, the stimulus is delivered in such a way that the objective moment of its occurrence can be recorded. Furthermore the inference is made that this moment is also the moment at which the subject becomes aware of the stimulus. The next measurable event is the onset of response, and the interval between these two events is considered to be the " reaction time". This characteristic sequence of events is not necessarily a feature of the stimulus series of ordinary skills, and it was not in the author's experiments.

It is evident that for the purpose of a general analysis of skilled behaviour the classical concept of reaction-time has serious limitations. The objections made by Bartlett (1947) that earlier experiments had deprived the signals presented of the character of seriality which signals in life normally have, have been met in more recent work. Other objections have had less consideration. Probably in all the skills of everyday life, some signals occur without warning and require immediate response. But very few such skills can be said to be characterised by this type of display. One doubts in fact whether response to these signals could usefully be termed " skilled ", since the subject's appreciation of the nature of the display would be minimal.

Furthermore, it is generally accepted that the skilled response is graded in time. That is, it is not made as hurriedly as possible after a signal, but that there is a moment when it is optimum. This implies that the achievement of the response at the optimum time is not rendered impossible through unfavourable temporal relationships between signal and opportunity for response. In simpler language, the subject must know in advance that a response will be needed if it is to be made at the right time.

When this condition is fulfilled, the inference regarding the subject's awareness of the signal, on which reaction-time theory has rested, is valueless. Logically stimulus must precede response. Yet in fact as has been shown, response is just as likely to precede what is *nominally* the stimulus as it is to follow the stimulus. It is clear that the actual psychological signal needs to occur some time before the nominal signal. In the task studied, the nominal signal was the moment of coincidence of pointer and middle of the target mark. It may very well be true that the processes convincingly postulated by Welford (1952) do succeed one another. But the one critical event which is unrecordable, is the moment of occurrence of the psychological signal which may be any time before, or even after, the nominal signal.

There is, therefore, one element which is frequently present in the anticipated-signal-response sequence which is not accounted for in reaction-time analysis. This is a period which can be assumed to lie between the end of the (classical) reaction-time and the beginning of the response. It is postulated as a delay which enables the subject to respond at the right moment. There is no direct and certain method of measuring the extent of this delay since it follows on, after a reaction-time, the unrecordable psychological signal. For this reason there is no obvious way of determining the influence of this period on other behaviour. In particular it is not known what, if any, elements of behaviour are inhibited during this period. Whether, for example, the reaction-time of one response can occur during the delay before another, or whether two delay times can overlap, or how much visual searching can proceed during this period. Yet the delay is of great importance in skill. It is the element of behaviour between the perception of a stimulus and the completion of response, the duration of which is most likely to be susceptible to variation. In so far as timing is voluntary, it is this delay which will probably provide the subject with the freedom to hasten or withhold the response.

TIMING

It was previously shown (Conrad, 1954b), that the temporal position of a response R2 depended on the time interval between its signal S2 and the previous response R1. The longer this interval was, the greater was the probability that the response would be made early. In particular this dependence did not cease when the relevant interval R1-S2 became longer than 0.5 sec., which is about the maximum duration postulated in reaction time theory for the occurrence of the necessary reaction processes in the prevailing conditions. Furthermore this relationship was independent of the time interval between S2 and the next signal, S3, which interval itself also affected the position of R2. With the position of a response influenced both by the time since the previous response and also by the time until the next signal, the suggestion seems justified that there was continuous psychological linkage between successive responses. This was apparently maintained even when the interval between them was considerably longer than the sum of the constituent components which might be expected from reaction-time theory. Thus it seems that before the reaction processes of R1 were completed, in a large number of cases the subject had begun those of R2. He had determined the next signal to which he would respond, and had evidently appreciated in relative terms how much time he had between it and the response being (or about to be) formulated.

Except when S2 was very close to R1—and this was largely determined by the interval between successive signals and thus outside the subject's control—according to reaction-time theory the subject would be free to make R2 whenever he wished. The evidence indicates that this freedom does not in fact exist. The position of a response results from adaptation to events both after as well as before it. There are, however, important differences between these two compelling factors. When the determining interval is S2-S3 the matter is beyond the subject's control. Where though it is R1-S2, it is not, since the subject himself fixes the position of R1.

It is obvious that the temporal position of a response cannot be absolutely determined, relative to the position of events before it and after. The previous response and the next signal establish forces which are not ignored. But the nature of the interaction between the subject and these forces is as much adaptive as compulsive. Thus in making one response, the subject himself partly determines the temporal conditions in which the next response will have to be made. Only partly determines, because the display itself may have an unalterable temporal character to which the subject must adapt if the skill is to be maintained. It is this close interaction between the skilled operator and the temporal structure of the display or environment which is characterised by timing. Timing therefore, is defined as *creating the most favourable temporal conditions for response*. Conditions for response are considered to be favourable when the response can be made with the least hurry, at the best moment, whilst leaving the organism in a satisfactory post-response state.

Drawing on evidence from Vince (1948) and Leonard (1950), Welford (1952) has shown that the "central organising time" of R2 could overlap with the movement time of R1. But if the responses were truly serial and the interval between R1 and R2 was long, either the central organising time and/or the movement time of R2 would need to be voluntarily prolonged, or the onset of the movement time of R2 delayed: The duration of a central organising time is of course only detectable by the observed onset of a movement, and it would be difficult, as it is at present conceived, to defend the thesis that it was under voluntary control. Movement time could easily be prolonged, but this would then be psychologically equivalent to a delay.

Whether movement is temporarily withheld or merely slowed down, the concept of a delay seems to be an essential link in any theory of the timing of skilled responses.

THE EFFECT OF TIME ON TIMING

One limitation on the subject's ability to maintain an effective level of timing is of course timing itself. There is no circularity here, any more than there is in the statement that the further one runs the more difficult running becomes. Bad timing in the present makes timing in the immediate future more difficult. Timing has been defined in terms of the ability to create favourable temporal conditions for response and some evidence for this has been cited. Bad timing will result in unfavourable temporal conditions for response and in such conditions timing will inevitably be more difficult.

Since timing as an activity is considered to rely primarily on perceptual data, it cannot always be directly observed. Good or bad timing, like many psychological processes, may have to be inferred from its effects. It has been stated by definition that bad timing would create unfavourable temporal conditions for response. But it should be remembered that the display itself, in paced tasks, plays an active role in this. It sets up the prior temporal conditions which sometimes will assist response and sometimes hinder. If one were looking for effects of bad timing, it should be most profitable to look where the display presents a difficult temporal framework. For similar reasons, car racing enthusiasts with a predilection for thrills will watch the race at corners rather than at straight stretches. It is equally reasonable to suppose that difficult conditions in skill would normally be found where signals were close together.

It may perhaps be argued that the most difficult conditions that subjects in fact meet, are those which result from a signal occurring close to a *response*; it is only in such cases that one could be certain that the subject was contending with, rather than withdrawing from the situation. A group of signals following closely on each other, but none of them responded to, give little information about the subject's behaviour. But a recorded response represents in general an objective link between subject and display at that time. It has been shown (Conrad, 1954a) that this is the kind of situation in which signals tend to be omitted, if they are missed at all. More than three-quarters of all omissions occurred within 0.5 sec. of a response. The further from a response a signal occurred, that is, the more favourable the temporal conditions around it were, the more likely was it to be dealt with. Thus it is worth while considering that omissions may be an indication of timing ability.

This hypothesis might throw light on previously presented data (Conrad, 1954a) which showed that for an interval between signal and response of constant size, the signal was more likely to be missed as mean signal speed increased. Bad timing may *show* in an omission close to a response. But bad timing is most likely to *occur* at faster speeds because there would be less time for timing, as for all other processes. In this case one would expect similar specific temporal situations to show omissions more frequently at faster than at slower mean speeds. Timing like vision is conceived of as forward looking. No matter where bad timing as a process is likely to occur, it will have overt effect more certainly at weak points. But it will occur with greater likelihood in conditions where there is too much to do in too little time.

If the hypothesis of the significance of omissions is valid this error might be considered to be an index of timing other than the accuracy of response in relation to its signal. The position of a response represents the subject's attempt at coincidence in time with an event in the display. But it has been shown, further, to represent an effort on the subject's part to creat favourable temporal conditions in the immediate future. It is easy to see that if the future were ignored, a very accurate response might prejudice the chances of the next signal being effectively dealt with. An omission on the other hand, is one way, sometimes possible, of keeping in touch with the display. It seems almost certain, that if very heavy penalties were associated with omissions, as the signal speed increased, subjects would completely lose control over the situation.

It should be recognised that in skill in general, failure to respond to a signal—even if it were possible to detect it—is not necessarily the first or the most important criterion of bad timing. There are skills (in the textile industry, for example) in which signals remain present in the display until they are dealt with. In such a case bad timing might show in unnecessary delay in dealing with signals. Whenever timing is an integral part of the maintenance of skill, bad timing will mitigate against the successful achievement of the aim of that skill. How it will show must depend on the nature of the display and of the responses required to adapt to it.

If it has been shown that timing is a process of skilled behaviour it is outside the scope of this paper to attempt to describe the mechanisms by means of which timing is performed. Elementally, it is fairly certain that most, if not all, of them, are well known. Without the use of sensory perception, of anticipation, of discriminatory processes, of memory span—all familiar in psychology—it is most doubtful whether timing could occur. In certain combinations they provide an ability without which one would be helpless in the face of an ever changing environment.

One further incidental point which merits attention refers to the concept of "human error" in skill. When some disaster on the road, rail or in the air takes place, enquiry often reports that it was due to error on the part of some individual who should have been in control of his part of the situation. It has been argued in this paper that in so far as the situation refers to a changing series of signals for action, or consideration, the immediate temporal character of the situation might vary very greatly within short time limits. In the course of this variation there might very occasionally occur a group of important signals, the temporal relationships between which were such that abilities would be required to deal with them far in excess of the limits of human ability. The error which resulted might very well be called human, simply because up to that moment the human being had been able to deal adequately with the occurring signals. In fact there might be no error, but accident in the statistical sense.

ACKNOWLEDGMENTS

I would like to record my thanks to the Royal Navy who provided most of the apparatus and the subjects for the investigation discussed in this paper, and to the Medical Research Council for their financial support.

The work was carried out under the direction of Dr N. H. Mackworth to whom I am deeply indebted. The inspiration of the teaching of Sir Frederic Bartlett will of course be obvious, and any gratitude that I can express for enjoying the benefit of his wisdom will inevitably be understated.

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Weak and Anomalous Colour Vision in Industry and the Need for Adequate Tests

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INTRODUCTION

It is important to realise the possible effects of anomalous colour vision in daily life and industry, because colour anomalous persons are serious defectives in comparison with normal people. They and the strict dichromats form small groups in the population, together totalling about 7 or 8 per cent of men and less than 0.5 per cent of women, statistically discontinuous with the normal group and not simply forming the tails or extremes of a continuous distribution (Rayleigh, 1881; Pickford, 1949a, 1951, 1954; but see also Chapanis and Halsey, 1953). Colour vision weaknesses of a minor kind must also be considered, because they are important for industry. They, in contrast to strictly anomalous defects, do form the tails of the distribusions of normal variations, and consequently the propotion of people who are considered to have minor defects is partly dependent n their efficiency in relation to the requirements of their tasks or occupations. Probably about 10 or 15 per cent of both men and women may be considered to have minor defects of sensitivity to red and green and/or yellow and blue (Pickford, 1951).

DEFECTIVE COLOUR VISION IN INDUSTRY

The industrial significance of colour vision defects have been dealt with in recent years by several writers, including Cavanagh (1955), the Colour Group of the Physical Society (1946), Hardy (1951), and earlier by Pierce (1934), whose insight into the problems of selection for training was considerable. In order to make the importance of colour defects in industry clear it will be valuable to take a concrete example. In a paper mill the precise hue or shade of colour of the huge sheet of paper being produced on the rollers is controlled by the following method. There is a standard sample of paper to be matched in a small side-room under a daylight lamp. Small pieces of the paper being made on the rollers to match it are taken at intervals and compared with this sample. If they do not appear to match the sample slight changes are made in the proportions of the dyes incorporated in the pulp accordingly, to counteract the deviations in its shade or hue. This process is carried out even in the manufacture of cream or grey paper, which may vary slightly in shade in comparison with the standard and must be corrected if it does.

In the final result, whatever the hue, shade or colour, the paper produced varies slightly around the norm or standard, in accordance with all the small changes which have been noticed and corrected, and, when it is cut into large sheets there are noticeable differences between some of them resulting from the deviations and corrections. Later the sheets must be sorted by trained operatives into piles in which all sheets are of the same hue or shade, or rather, into piles in each of which the deviations are no longer noticeable. Batches thus sorted may be said to be of the same colour, but, of course, some batches inevitably deviate very slightly from the standard.

Both essential steps in such a process require first class colour vision, because the tolerance of the operatives for colour deviations even of a very small kind depends on their sensitivity to slight hue differences and on their capacity to be trained to make the necessary discriminations. At either stage a colour blind person or a colour anomalous subject would be useless, but would probably be detected easily because he or she would make gross errors easily noticeable to anybody with normal colour vision. At each stage a person with any minor weaknesses of colour vision would be inefficient, and his judgments might not satisfy the fastidious or he might be abnormally difficult to train. Similar considerations apply to other industrial processes in which fine gradations of colour are involved, and therefore a consideration of the effects of anomalous colour vision and minor colour vision weaknesses in daily life and of the need for adequate tests is of some practical interest.

TYPES OF COLOUR VISION DEFECTS

The following distinctions between types of red-green major defects, namely those which do not form parts of the normal distribution of colour vision variations, will be assumed, since they have been established by other work (Waaler, 1927; Franceschetti, 1928; Pickford, 1951, 1954; Willis and Farnsworth, 1952). The strictly anomalous (protanomalous, PA, and deuteranomalous, DA) have very large deviations to the red or to the green side respectively in the Rayleigh Equation (Rayleigh, 1881) in the anomaloscope, with little or no increase in matching range compared with the normal. The extreme protanomalous (EPA) and extreme deuteranomalous (EDA) have very large increases in matching range with or without marked, moderate or slight deviations of mid-matching point. Protanopic and deuteranopic dichromats (P and D) have no measurable deviation but extreme matching ranges so that they can match any mixture of the red and green lights, or even the red or green taken alone, with the yellow standard, provided it is equated in intensity with the matching colour for each individual being tested. In contrast to deutans (DA, EDA and D) all protans (PA, EPA and P) with the exception of certain PA subjects, have a very great darkening or shortening of the red end of the spectrum, though the degree of darkening or shortening is apparently not correlated with the degree of red-green colour vision defect. Certain unusual forms of red-green major defect are confined to a central scotoma, the testing of which is mentioned by Sloan (1942) and Pickford (1951).

Minor weaknesses of colour vision are essentially of the same kinds as mentioned above, but are very much slighter, and they belong to the extremes of the normal distributions of colour vision variations. They affect blue and yellow as well as red and green (Pickford, 1951). The importance of testing for yellow minor weaknesses is quite independent of any controversy about the theories of three or four primaries, because, however it may be explained theoretically, yellow sensitivity varies in ways which cannot be measured by tests limited to red, green and blue (Pickford, 1951). There are rare subjects of the tritanopic or blue-defective class, who might be called major defectives (Wright, 1952), and also rare achromatic defectives who are totally colour blind (Walls and Mathews, 1952, Walls and Heath, 1954, and Sloan, 1954).

The importance of an adequate grasp of the statistical distributions of colour vision variations can hardly be over-estimated. In the distribution of deviations in the Rayleigh Equation there are marked gaps between the most deviant of the normal and the least deviant of the red and of the green anomalous. In the distribution of matching ranges there are marked gaps between the weakest of the normal and the least weak of the protanopes and deuteranopes. It is the existence of these gaps which makes testing for colour blindness by lantern and pseudo-isochromatic tests practicable for excluding major defectives. The whole problem is completely different from the problem of testing for mental deficiency and low grade and borderline intelligence, in which we are dealing with a continuous distribution of intelligence quotients. Testing for small gradations of colour vision variation, either on the normal group or in any of the groups of major defectives, calls for more discriminating tests, of which the anomaloscope is the most simple and practicable.

ANOMALOUS COLOUR VISION IN DAILY LIFE

Subjects of the strictly protanomalous and deuteranomalous groups often go through life without noticing any abnormality, if they are not given searching tests. They have normal or almost normal matching ranges, and deviations in the Rayleigh Equation even as great as theirs do not necessarily lead to gross errors under ordinary conditions, but these subjects often betray their defects or arouse suspicions in the minds of experts by holding strong views of the kind, for instance, "that it takes a woman to judge greens", and they often show a special resentment in their manner of expressing these views. Some who are more self-critical than usual realise they have different colour vision from the majority. One girl, an art student who had the characteristic DA defect, explained that she had a "different scale of colours from that of other people". A PA subject knew that he had misjudged purples and violets in the pastels in the school art class.

On account of the possibility of their going through life without knowing of their defects, PA and DA subjects, far from being "safe defectives" are particularly dangerous, because they tend to have confidence in their own colour judgments, and even resist any suggestion that they are not normal. They are often indignant at being failed in tests, and the writer has had the task of explaining to a number of them who had been rejected by the Air

Force, that, although they were not "colour blind" they were capable of making mistakes under unusual conditions and that such mistakes might endanger other people's safety or even cost lives. There is nothing so convincing of this as to set the anomaloscope at the normal matching point and then to ask a red and a green anomalous person to comment on the setting and discuss it with each other. The red anomalous subject will call the standard yellow colour "red" and the variable mixture "green", while the green anomalous will call the yellow standard "green" and the variable mixture "red", and the tester, if he is normal, will call them both "yellow". It is then clearly evident that dangerous mistakes might be made, especially under unusual and critical conditions.

Those who are extreme protanomalous (EPA) and extreme deuteranomalous (EDA) may be considered in two groups. When the matching range is relatively small for a major defective, though still very large compared with the normal range, and when there is little or no deviation in the Rayleigh Equation, more probability exists of the subject knowing that he is a defective than in the strictly PA and DA classes. This is because an abnormal matching range tends to lead towards colour confusions, whereas a big deviation leads rather to changes in the subject's scale of colours compared with that of the normal. The subject with the abnormal matching range therefore makes more noticeable errors than the subject with the large deviation. He generally suspects a defect secretly, and may distrust his colour judgments, which is a safeguard. His relatives may know definitely that he has difficulty in judging colours, and may say that he is "incapable of arranging flowers", or laugh at him occasionally for some mistake he is known to have made, as with a girl whose mother laughed at her for not being able to buy her own lipstick. Subjects of these types must be rejected in any serious colour vision test. The only question to decide is at what magnitude an abnormal matching range is seriously dangerous. Since there is more continuity between the normal colour weak and the least defective of the EPA and EDA, than between the normal deviant and the least defective of the PA and DA, this may be a difficult problem for practical research.

The EPA and EDA subjects in the second group, namely those who have very large matching ranges, some being almost completely dichromatic and some having large deviations as well, generally know in their own hearts that they are abnormal, though they and their relatives often think that they have never had a proper opportunity of learning colour names and could do so if given a fair chance. One boy's mother explained that he was brought up in the country and had in consequence never learned to name colours properly, but it did not occur to her that most country boys have no such difficulty, and he proved to be a seriously defective EDA subject. He was capable of learning the colour name as words, but not of applying them correctly.

There should be no question whether anomalous subjects of any type might be given the benefit of the doubt in any serious test of colour vision.

When we come to consider the extremes of the normal group, namely the deviants and the colour weak (Pickford, 1951), it is another matter, and in general they can be passed for all but fine colour matching work. The anomalous, however, should be detected by a combination of a good pseudo-isochromatic test, and Rayleigh Equation applied with a competent technique and with an adequate anomaloscope. Deviation, complete matching range and brightness of the red end of the spectrum must all be measured. Subjects of the anomalous types should be excluded from all occupations in which colour vision is of practical importance, especially where life depends on colour signals.

THE PROBLEM OF ADEQUATE TESTS

In view of the dangers possibly caused by anomalous colour vision in daily life, the need for adequate tests and efficient testing techniques can hardly be over-estimated (Wright, 1953; Hardy, 1951). A recent discussion on colour vision in industry by Riddell and others makes this abundantly clear (Riddell *et al.,* 1949). Simple cut-out tests like Holmgren's Wools, which fail dichromats and the more extreme anomalous subjects, but pass some anomalous subjects together with the normals, were criticised by Edridge-Green many years ago. Other cut-out tests, like the Ishihara Test and other pseudo-isochromatic plates and tables, which generally fail all dichromats and all types of anomalous subjects almost in a single block, and on which the number of errors bears little or no relation to the degree or kind of defect, are liable to give rise to many confusions, and although they have much practical use they are relatively undiscriminating (Cavanagh, 1955; Chapanis, 1949a; Hardy, Rand and Rittler, 1949, 1954; Pickford, 1949a, 1949b, 1950, 1951; Sloan, 1945, 1951).

The Rayleigh Equation (Rayleigh, 1881, Edridge-Green, 1920; Houstoun, 1932; Collins, 1929; Pickford, 1949a, 1951; Crawford, 1951) has been the most useful and discriminating of all simple colour vision tests, but must be used with insight and an adequate technique if it is not to mislead. For example, to take the mean of three settings made by the subject gives no opportunity for the study of his range of matches, and this may be more important than his deviation. Since many extreme anomalous subjects have no deviation, it is hardly surprising that some workers have said that the Rayleigh Equation does not measure the degree of defect of anomalous subjects consistently, and obviously a more efficient way of using the test is required. Sloan (1950) points out that the Nagel Anomaloscope (which employs the Rayleigh Equation) might miss a certain number of defectives. The normal setting is acceptable to all dichromats and to many extreme anomalous subjects as well, and unless the whole range of matches is carefully determined, brightness being adjusted for every colour judgment, a normal or almost normal setting is certain to be chosen from time to time by some P, EPA, D or EDA subjects, by chance if not by intention.

In order that the Rayleigh Equation in any anomaloscope should be used effectively it is necessary that the controls should be in the hands of the tester, not of the subject being tested (Pickford, 1951). The method of free adjustment by the subject is not satisfactory and should be avoided. It gives the subject too ready an opportunity to deceive the tester, and, quite apart from the unconscious "dodging" at which colour vision defectives are experts, it is not inconceivable that a defective subject might have learned to make a normal setting without using colour as a guide at all. At the same time this technique fails to give the tester a proper opportunity of establishing all the matching points, and unless this is done the subject's degree and kind of defect may not be detected efficiently (Pickford, 1951; Willis and Farnsworth, 1952).

To arrange two lights to be compared in the same field of vision precisely side-by-side, so that the subject may examine them at leisure while fixating a spot on the line between them, may make the test less efficient, because retinal fatigue soon sets in and tends to approximate the hues being compared in the two halves of the test field. Such an arrangement will also be affected by simultaneous contrast, and this, in turn, will depend greatly on the sharpness and width of the line, if any, dividing the field into two halves. Tests based on the measurement of sensitivity to single hues may possibly mislead because of the importance of deviations due to the bipolarity of the red and green receptor system, shown clearly in the Rayleigh Equation, but to which simple threshold tests may not be sensitive.

All tests based on polychromatic filters or coloured papers may have low reliability and/or validity. In the Nagel anomaloscope, which uses spectral lights, the increase in saturation of either the red or the green relatively to the other colour in the mixture which matches the standard yellow is obtained by adding new wave lengths by widening one slit and narrowing the other. The bigger the deviation the less homogeneous one of the lights in the mixture. We do not know whether a disadvantage, if any, is incurred in this way. Houstoun's apparatus (Houstoun, 1932) and the Hecht-Schlaer anomaloscope and some other forms of the apparatus (Willis and Farnsworth, 1952), use a polarization mechanism for the differential change of red to green in the mixture, but even simpler mechanisms may be used (Crawford, 1951; Pickford, 1949a, 1951) and the Ilford monochromatic filters are excellent.

Tests based exclusively on the naming of hues by the subject, or on his description of what he sees, whether the tester can see it at the same time or not, are too subjective to be satisfactory by themselves. Naming by the subject, as Edridge-Green pointed out, is a desirable part of a colour vision test, but should be taken in conjunction with objective results. It is important that the tester should be able to see what the subject sees at the same time, and be able to check his judgments and to use clinical experience to help in assessing the significance of the subject's performance. Nobody will be a good psychological tester of any kind who is not a good clinician. Colour vision defectives are far too artful in unconscious and habitual ways of testing the tester for tests to be carried out in a mechanical way or as if they were simply the equivalent of so many physical measurements. It is probably not possible to say beforehand upon what principles a test ought to be constructed, or what physical quantities it ought to measure, because we do not know on what principles the processes of colour vision operate, and, in any case, testing colour vision is a very different matter from testing a physical apparatus. Group tests are useless, but certain methods for mass testing have been employed (Murray, 1948).

The use of the so-called "exaggerated simultaneous contrast" of some anomalous subjects as a criterion, as explained by Lohmann for instance (Lohmann, 1913), in his use of the Nagel test cards, is entirely unreliable because the "exaggerated contrast" is probably an effect of judgment rather than of colour sensitivity (Chapanis, 1949b; Pickford, 1951).

THE NEED FOR EMPIRICAL RESEARCH

The whole subject must be given painstaking research and empirical study directed towards establishing the reliability of tests on repetition and their validity in the sense of their capacity to distinguish whatever differences it is important to discriminate, without prejudice in favour of any particular theory. Validity and reliability must be determined operationally, that is, experimentally, and are not necessarily or always to be established satisfactorily by utilising certain methods simply because they are beyond cavil from the physical standpoint.

In their technique for using the Nagel anomaloscope Trendelenburg and Schmidt developed an interesting method for enabling the EPA and EDA subjects each to give a single decisive equation, like the normal and strictly anomalous (PA and DA) who have very small matching ranges. In this method the subject is asked to fixate a standard diffused white light, which is attached to the base of the instrument, before carrying out the test. Further research is needed to find out the relationships between the anomaloscope quotients obtained in this way and the mean deviations obtained by the method of measuring the entire matching range. The problem is discussed in an interesting way by Walls and Mathews (1952).

Willis and Farnsworth (1952) have made an important contribution to the solution of problems of the empirical study of anomaloscopes. Unfortunately in their comparison of six instruments a truly random and representative sample of the population being tested was not used. Another difficulty in their work is that they have assumed that it is possible to equate the end-points of the scales of the six anomaloscopes although the instruments differ widely in the physical properties of the coloured lights being used. Manifestly this is not readily acceptable in principle though it may not affect results greatly. These workers indicate that there is considerable uniformity in the results obtained with adequately designed instruments, even though spectral lights are not used. The more widely the red and green colours are separated on the colour mixture diagram, and the narrower their wave transmission bands, the better the test, but it is essential that the anomaloscope should enable the intensity and perhaps also the saturation of the yellow standard to be under control. Other researches have also been aimed at the empirical comparison of various colour filters and techniques of testing in the use of the anomaloscope (Pickford, 1951). The new "interference colour filters", which have great efficiency combined with very narrow transmission bands, may make filter anomaloscopes as satisfactory to the physicist as the Nagel instrument which uses spectral lights.

It is customary in testing for certain purposes to divide candidates on the strength of a lantern test such as the Edridge-Green Lantern or the Giles-Archer Colour Perception Unit, in conjunction with the Ishihara Test or other pseudo-isochromatic tables, into (a) colour normal, (b) colour defective safe and (c) colour defective unsafe. It would be interesting to find out empirically the kinds of subjects, according to the anomaloscope, who are classed as "colour defective safe" on the Giles-Archer lantern. The Ishihara Test, and several other types of pseudo-isochromatic tests based on similar principles, have been studied many times in different researches, both in comparison with each other and in comparison with more discriminating tests of the anomaloscope type, as by Collins (1937), Chapanis (1948, 1949a), Hardy, Rand and Rittler (1945, 1946, 1947, 1949, 1954), Sloan (1945), Sloan and Altman (1951), Pickford (1949b, 1950, 1951), Willis and Farnsworth (1952), Ross and Fletcher (1953)and Crawford (1955). Lantern-type tests have also been studied by similar methods (Colour Vision Requirements in the Royal Navy, 1933). In general it is clear that these tests all act as cut-outs operating at the gap between the normal distribution of colour vision minor variations and the groups of major defectives (P, EPA, PA and D, EDA and DA), with varying degrees of validity and reliability. Considerable claims are made for the H-R-R plates (Hardy, Rand and Rittler, 1954), but the anomaloscope must be used for efficient measurement of both minor and major defects.

MINOR DIFFERENCES OF COLOUR SENSITIVITY

The testing of minor colour vision weaknesses may be important in many industries, such as colour printing, dyeing, and paper and linoleum manufacture. In a linoleum factory the Lovibond Tintometer was used for measuring the differences between a standard sample and the product being manufactured to match it. When asked whether there were differences of opinion among operatives all of whom had been passed by the Ishihara Test, the management replied that there were such differences. They were forced to give the task of comparison and measurement always to the same man, who was presumably colour normal. The Ishihara and pseudo-isocromatic tests are not capable of detecting the minor differences of sensitivity involved (Pickford, 1951). Pierce (1934) discussed the problem of testing recruits for training in fine colour discrimination in order to weed out beforehand those not capable of acquiring the necessary colour vision skill, and the difficulties he met with in his attempts to make an adequate test are very instructive, although he failed. A form of anomaloscope in which at least four colours, namely red, yellow, green and blue are used would be necessary for this purpose (Pickford, 1951), and would be valuable in industry if properly handled. It can be shown, for instance, that the frequent disagreement about whether such colours as Cambridge Blue, Eau de Nil, Electric Blue and other familiar desaturated blue-greens, are really blue or green, is due to differences in colour sensitivity. Those who insist that these colours are green are minor blue defectives. Probably minor blue weaknesses increase about middle age owing to the yellowing of the transparent parts of the optical system (Riddell *et al.*, 1949), but this has never been proved in detail by adequate experimental researches. The relationship of major red-green colour vision defects with age has been studied by Chapanis (1950), whose findings indicated no change with age, as did those of Brown (1951), for major and minor variations.

CONCLUSIONS

Anomalous colour vision, far from being a 'safe' defect, is a decided danger wherever coloured signals are employed, and has important effects on colour discrimination in industry and in daily life. Minor colour vision defects are also of considerable importance and ought to be thoroughly tested in many industries and factories.

Adequate tests should be developed for the detection and study of weak and anomalous colour vision. Some form of anomaloscope will be best for this purpose, but adequate empirical research is required, carried out on representative random samples of the populations tested, and utilising various forms of test and techniques of measurement.

Such research will enable us to perfect the methods and apparatus used, and to determine the types and degrees of defect more efficiently, and to establish their relative frequencies in the various populations tested.

While pseudo-isochromatic tests are capable of failing all major red-green defectives, including the colour anomalous, in a block, indiscriminately, for the measurement of minor and major deviations of colour vision the anomaloscope is essential. No lantern test or pseudo-isochromatic test is capable of discriminating or measuring the small variations of sensitivity involved, although such differences may be important in industry.

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Porebski's Theory of Speed and Power Factors of Intelligence

By D. F. VINCENT

In this paper Mr D. F. Vincent, of the Test Section of the Institute, criticises the theory of speed and power factors of intelligence advanced by Dr O. Porebski in a recent issue of this Journal.

THE idea that human ability should be measured in terms of height or level as well as in terms of speed was advanced by Thorndike as early as 1926, and the subdivision of intelligence tests into power tests and speed tests has been in use for a very long time. These terms, however, are usually used to distinguish between methods of testing, and do not imply that the two types of intelligence test measure different traits of intelligence. The idea that there are separate power and speed factors is at variance with present day views, but this is in fact what is suggested by Porebski (1954).

This paper sets out to re-examine Porebski's arguments and to suggest that his conclusions do not necessarily follow from his experimental findings, and that his findings are not greatly at variance with what would be expected on the basis of the more generally accepted theories about the structure of human abilities.

Porebski's main evidence is a table of the intercorrelations of nine tests, three of which, made up of items using drawings rather than words or numbers, he calls Spatial tests; three others, consisting of items presented in the form of words, he calls Verbal tests; and the remaining three, consisting of items involving numbers, he calls Numerical tests. He uses a second classification of the tests into Speed tests and Power tests. There is one Spatial Power test, one Verbal Power test, and one Numerical Power test, and two Speed tests of each type. The Speed tests were administered as group tests, the times allowed being from 1½ to 4 minutes. The Power tests were taken home and returned a week later usually, but in cases in which the testee wanted more time another week was allowed. Basically the Speed tests were short timed tests each consisting of twenty-five relatively easy items, whereas the Power tests were untimed tests each consisting of two or three relatively difficult items.

From the evidence of the intercorrelations Porebski advances two arguments. The first is based on the average intercorrelations between the tests when they are grouped in five different ways, *i.e.* the average r between Power tests, the average r between Speed tests, the average r between Verbal tests, the average r between Spatial tests, and the average r between Numerical tests. In the absence of evidence about the reliabilities of the various tests, however, this argument is inconclusive. Although Porebski treats the first argument as separate from his second one, which is based on the factor analysis of the matrix of correlations, it is in fact merely a tentative approximation to the factor analysis argument, and need not be treated separately.

Before dealing with his argument through factor analysis, it is necessary to consider the reliability of the correlation coefficients forming the experimental evidence. If all had been calculated by the product moment method, there would have been standard errors up to .14. Only 15 of the 36 coefficients, those of correlations between the S tests, could have been calculated by the product moment method. As is explained at the top of page 222 of his paper, the P tests were scored by a three class classification, so that none of the correlations involving the P tests could have been product moment correlations. Some, it is stated, were contingency coefficients. In any case 21 of the 36 coefficients in the table must have higher errors than those of a product moment coefficient.

Porebski's main argument is that his correlations can be analysed into Speed and Power factors whereas they cannot be analysed into Verbal, Spatial and Numerical factors. He makes no mention of a general factor. It is quite true that his table of correlations cannot be resolved into *g*, *v*, *k* and *n* by the centroid technique, but that is because the errors of his coefficients are so high that it is not possible to take out more than two centroid loadings. The second set of loadings must contain a high proportion of error, a third set would be an artifact conjured up out of the sampling errors. For this reason it would be quite impossible to resolve his matrix into *g*, *v*, *k* and *n*, four factors, by the centroid method.

There is another reason for the non-appearance of group factors. For the Spatial tests S3 and S4 Porebski has chosen item types which are favourites for intelligence tests. They are used for intelligence tests because it has been found that they have a high *g* loading and a negligible *k* loading. An example from the third Spatial test, P2, is shown on page 231; the other two items were similar but easier. These items are of the type employed in the well-known 1938 Progressive Matrices Test, though two of them are harder than any item in that test.

Of the Numerical tests, S5 is a straightforward arithmetic test, S6 is of the 'continue the series' type, and P3 consists of three questions similar to the example shown on page 231. Most of the variance of arithmetic tests is due to *g* and to arithmetical knowledge; they are not usually used as *n* tests. The item types of S6 are mostly of the kind used in *g* tests. An item involving a number series is not necessarily a numerical question. Series such as 1, 2, 3, 4 . . . and 20, 18, 16, 14 . . . do not require any particular numerical ability for them to be recognised as known sequences. Questions such as 'Continue the series 1 2 2 3 3 3 . . .' or '20 1 19 2 18 3 . . .' are not changed in kind if a known letter sequence is used in place of a known number sequence, as for instance, A B B C C C . . . and Z A Y B X C . . . Of the three questions in test P3, the last is shown on page 231, the second is similar but somewhat easier, the first is very much easier and can readily be solved by inspection. The second and third questions can also be solved by inspection, but in the writer's experience that was a very tedious way of solving them. They can be solved very much more easily and quickly by a straightforward mathematical operation*, but mathematical knowledge is not *n*.

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* The writer used the device described as 'vanishing triangles' or as 'difference series' in text books of algebra, but there may be other ways.

On the whole it does not seem likely that Porebski's tests have anything but very small loadings of k and n. It would require the scores of a thousand cases rather than fifty to obtain correlation coefficients with sufficiently small errors to detect these factors in a centroid type analysis. Tests S1 and S2 consist of item types known to have v loadings; whether there is any v in test P1 is not as certain. The v factor was probably present in Porebski's matrix, though for the reasons stated it is unlikely that it could be detected by a centroid type of analysis. There is the possibility that evidence of v if not of k or n could be found by a Bi-factor analysis.

A Bi-factor analysis was made of Porebski's correlations, dividing the tests up into three groups, the Verbal tests S1, S2 and P1, the Spatial tests S2, S3 and P2, and the Numerical tests S4, S5 and P3. After the removal of a general factor each of the three groups was found to be a Heywood case; that is to say, the residuals could be resolved into a single factor only by giving one of the tests a factor loading greater than unity. Usually this indicates that more than one factor is present, but as there were only three tests in each group and the standard errors of the original correlations were known to have been high, it is more likely that the Heywood case state of affairs is a spurious effect due to the errors. The Bi-factor method does not seem to be any more satisfactory than the centroid for demonstrating the presence of group factors.

It seems fairly certain that the data are inadequate to determine whether any other factors are present apart from the two found by Porebski. There does not seem to be much validity in the claim that Speed and Power factors are more fundamental than the more familiar g, v, k and n, because v, k and n cannot be detected by factor analysis. There remains, however, the meaning that is to be attached to Porebski's two factors and that, in the writer's opinion, is of considerably greater interest than any claim as to their fundamentality.

Porebski's resolution of the centroid loadings into a speed and power configuration springs from his choice of 38° for the rotation. Had he followed the Thurstone principle of rotating to obtain the maximum of zero, or non-significant loadings he would have used a rotation of about 26°. Table I below shows the loadings when the axes have been rotated through 26°, in accordance withThurstone's principle.

TEST	g	X	g	X
S1	·722	·186	·72	—
S2	·672	—·067	·67	—
S3	·800	—·016	·80	—
S4	·511	—·172	·51	—
S5	·251	·297	·25	·30
S6	·714	·191	·71	—
P1	·418	·688	·42	·69
P2	·428	·625	·43	·63
P3	·321	·657	·32	·66

TABLE I

The first pair of columns show the actual rotated loadings obtained, the second pair of columns show the loadings rounded off to two figures, with loadings below .2 omitted as non-significant. The factor pattern that emerges is consistent with current theory. All the S tests except S5 (arithmetic) have significant loadings of the first factor only; this is undoubtedly *g*, in view of the items types used in these tests. The arithmetic test has a doubtfully significant loading, .30, of the second factor.

It is not very easy to find an interpretation of this second factor, X in Table 1, either from the data in Porebski's article or from an examination of the tests used. With the possible exception of the arithmetic test it is associated entirely with the P series of tests, and these tests are an unknown quantity.

The X factor appears to be something inherent either in the nature of these tests or in the method of administration; it is something new and interesting. If it were inherent in the nature of the tests it could be of the nature of a higher level or 'power' *g*, as claimed by Porebski. If it were inherent in the method of administration it would be more likely to be due to some trait of temperament. It was thought that further work on the X factor was justified.

Porebski had employed the highest coefficient in each column as the communality. This is a time-saving device that gives results that are usually accurate enough with a large matrix, but with the small matrix obtained with only nine tests it can it can introduce a certain amount of error. The tendency is to exaggerate some of the loadings.

The matrix was therefore refactorized using the iterative technique advocated by Sir Cyril Burt to find the best fitting values for the communalities, and the rotation was made for the maximum number of near-zero loadings. The rotation used was 29°. The results are shown in Table 2. In the last pair of columns, the loadings are rounded off to two figures and values below .2 are omitted as non-significant.

TEST	CENTROID LOADINGS		ROTATED LOADINGS		LOADINGS ROUNDED OFF TO TWO FIGURES	
			G	X	g	X
S1	·726	·082	·673	·282	·67	·28
S2	·532	·300	·612	·000	·61	—
S3	·711	·334	·788	·046	·79	—
S4	·323	·373	·468	−·167	·47	—
S5	·318	−·084	·238	·231	·24	·23
S6	·721	·100	·626	·266	·63	·26
P1	·677	−·440	·372	·729	·37	·72
P2	·647	−·381	·374	·652	·37	·65
P3	·529	−·356	·284	·571	·28	·57

TABLE 2

As was expected there is a small reduction in some of the loadings. Two other tests, S1 and S6, now have doubtfully significant loadings of the X factor. The new factorization shows that little trust can be placed in these small loadings, they appear or disappear according to the factorizing technique employed. This usually happens when the matrix is small and the errors of the correlations are large. The general pattern, however, is the same, the S tests have loadings of *g* and little else, while the P tests have all fairly large loadings of the X factor.

It was decided to repeat Porebski's experiment. Mr P. E. Reeve, a member of the N.I.I.P. staff, had given the tests to forty-two people in the industrial field, of whom all but one were university graduates, and that one was believed to be of a similar level of ability; it seemed likely that this group was of comparable ability with the group described by Porebski in the paragraph headed 'sampling' on page 221. Forty-two cases is a very small group and the inter-correlations of the scores will, of course, have errors comparable with those of Porebski's experiment. A group of this size does make the experiment very similar to Porebski's, but that was not the reason for its smallness. The P tests make very considerable demands upon a person's time so that volunteers are not easy to obtain.

The scoring of the S tests was quite straightforward, but there was some difficulty about the scoring of the P tests. At the top of page 222 we read: "The system of scoring the Power tests was originally based on the level of difficulty. With such tests used for the first time, the level of difficulty could only be assessed on *a priori* grounds. Thus it was felt that, instead of using original scores, it would be better to group the answers into three classes so that the top class comprised complete solutions, the medium class partial solutions, and the bottom class no solutions or unsatisfactory solutions". The writer found that this subjective method of scoring raised some difficulty. It seemed to him that the distinction between a 'partial' solution and 'unsatisfactory' solution was quite arbitrary, and that it was most unlikely that his judgment would always be the same as Porebski's. He therefore used a straightforward marking of one mark for each correct answer. Probably it did not make any very great difference since there were in all the questions (with the exception of the last question of P2) comparatively few wrong answers. In most cases a person either gave the correct answer or no answer at all.

There was some difficulty over question 3 of test P2, which is shown at the top of page 231. It was found that nobody had obtained Porebski's solution, though a number had put forward an alternative solution. Since those who obtained this second solution had certainly done better at the test than the other members of the group, it was decided to give a mark for it.

The correlations are shown in Table 3. As in Porebski's experiment, the 'correlations' of the P tests are actually contingency coefficients multiplied by a factor to make them comparable with correlation coefficients. The factorization was done by an iterative method to find the best fitting communalities, and the rotation was made for the maximum number of near zero loading.

	S2	S3	S4	S5	S6	P1	P2	P3
S1	·400	·423	·060	—·040	·154	·296	·127	·161
S2		·192	·090	·133	·274	·146	·135	·019
S3			·256	·112	·183	·173	·118	·044
S4				—·165	·052	·066	—·044	·076
S5					·361	·025	·119	—·015
S6						·015	—·198	—·132
P1							·760	·821
P2								·762

TABLE 3

The factor loadings found are shown in Table 4. In the last pair of columns the loadings are rounded off to two figures and again loadings below .2 are omitted as non-significant.

	CENTROID LOADINGS		ROTATED LOADINGS		LOADINGS ROUNDED OFF TO TWO FIGURES	
			g	X	g	X
S1	·489	·224	·520	·127	·52	—
S2	·420	·337	·536	·000	·54	—
S3	·460	·367	·586	·000	·59	—
S4	·108	·066	·126	·013	—	—
S5	·148	·096	·175	·014	—	—
S6	·201	·498	·468	—·262	·47	—·26
P1	·782	—·510	·290	·890	·29	·89
P2	·565	—·568	·084	·795	—	·80
P3	·548	—·632	·030	·837	—	·84

TABLE 4

The factor pattern that has emerged is very similar to that obtained from Porebski's correlations shown in Table 2. The chief difference is that the *g* loadings are smaller. The loadings for tests S4 and S5, which were small in Tables 1 and 2, have now shrunk below the level of significance. Again significant X loadings are found only in the P tests.

The shrinkage of the *g* loadings—they are all about three-quarters of those in Table 2—caused some surprise. Since the same tests had been used with the same instructions, and they had been given by a competent administrator, the only explanation appeared to be that Mr Reeve's group was in some way more homogeneous than Porebski's. This is presumably due to the fact that Porebski's group consisted of two sub-groups, N.I.I.P. Staff and university undergraduates, whereas Mr Reeve's group was essentially one population of industrial technical staff.

What is more interesting is that, despite the difference in the group and the difference in the marking of the P tests, essentially the same factor pattern has emerged. It certainly seems that the X factor is not an artifact, it is something real.

The nature of the X factor is to be found in the distributions of the scores in the P tests. They are shown in Table 5.

TEST	PEOPLE WHO ATTEMPTED NONE OF THE ITEMS, OR GAVE UP BEFORE GETTING ANY ANSWERS	NUMBER OF PEOPLE GETTING 0, 1, 2 OR 3 ITEMS CORRECT				TOTAL OF ALL WRONG ANSWERS IN EACH TEST
		0	1	2	3	
P1	11	0	4	27	—	3
P3	9	0	2	5	26	9
P2	10	3	6	23	0	44
				13*	10*	34*
				23†	—	12†

Each test was given to the same 42 people.

*Alternative solution accepted as correct.

†Question 3 omitted.

TABLE 5

There were only two questions in Test P1. Of the 42 persons in the group, 11 made no attempt, or gave up the attempt to solve either question, and 27 gave correct solutions to both. The other four gave only one correct solution; and, of these four, one person did not attempt one of the questions. Thus almost everyone who spent enough time to find the solutions found the correct solutions. The scatter of the scores was almost entirely due to the fact that about a quarter of the group either made no attempt to solve the problems or gave up the attempt before they found any solutions. Test P3, which has three questions, shows almost exactly the same state of affairs. So does test P2, if the last question is ignored.

In each of the P tests, the group tested seems to have sonsisted of two classes; those who did not attempt any of the problems, or who gave them up before they found any solution, and those who persisted until they had found a solution which in almost all cases (except question 3 of P2) was the correct one. Success at these problems seems to depend upon a person's inclination to solve them rather than intelligence. It is not very surprising that the P tests were found to have negligible *g* loadings.

All the people in Mr Reeve's group were asked to note down the time taken over each problem, and all but a few of them did so. The first question of P3 seems to have been the easiest, the times taken were from one to ten minutes. The first question of P2 seems to have been the next easiest with times from one to fifteen minutes, with the exception of one person who took thirty minutes. All the other questions were considerably harder and most of the times taken were from half an hour to two hours, though some took less than half an hour and two people took as much as four hours. Thus it will be seen that those who did solve these problems had to spend a very considerable amount of time over them, and it is not unexpected that in the case of each test about a quarter of the group were not disposed to spend all the time that was necessary to solve these problems. The important requirement for success seems to be sheer persistence, though the person's reaction to the types of problems will also affect the time that he will feel disposed to spend on them. One person wrote on his test sheet, " P1 and P3 represent the type of puzzle I gave up bothering about many years ago"; and this was not because he was unable to solve the problems, for he got six correct solutions out of the possible eight. No one gave the correct solution to the last question of P2.

The P tests, in the way in which they were administered, appear to be partly a measure of persistence or determination, and partly a test for the puzzle-solving attitude of mind. The X factor is probably a complex of the two. This factor is undoubtedly inherent in the way these tests were administered, not in the nature of the tests themselves.

The writer would like to express his thanks to Mrs R. D. Lancashire of the N.I.I.P. staff for her help with the calculations.

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Book Reviews

Factor Analysis for Non-Mathematicians. By C. J. ADCOCK. Melbourne University Press; London: Cambridge University Press, 1954. Pp. 88. Price 17*s*. 6*d*.

This book is intended for students of psychology who may be led to neglect factor analysis because of the mathematical complexity of current texts. It sets out to cover the essential processes of factor analysis in such a way as to make clear the underlying logic in simple language.

After introductory chapters on measurement and correlations Dr Adcock explains about factors, then about the principle of rotation and cluster-directed analysis. He goes on to discuss finding simple structures and combining centroid and group analysis. The book concludes with chapters on interpreting the results and on the role of factor analysis.

The aim of the book is laudable, and it may well succeed in reducing for some the cost, in effort, of acquiring an understanding of factor analysis. It is clearly written and short enough to skim through at a sitting. Even working through it with a pencil and paper, as one must do to understand fully a book of this type, should not be too onerous a task.

However, it will not bring applause from all factor analysts, and the reader should be warned of its main shortcoming. One would have liked to see in this book some recognition and a more balanced assessment of the fundamental and important work done by Spearman in this field. Certainly, it is mentioned in passing that Spearman "first called our attention to this sort of situation"; but the later excuse "we shall not consider (Spearman's method of analysis) here, because it is now never used" strikes one as a polite way of being partisan. The author takes pains to describe clearly the methods of Burt and Thurstone, but three pages only are devoted not to description of Spearman's method, but to explaining why it is useless anyway.

Such scant treatment for the inventor of factor analysis does no credit to the science. Were carpenters to argue that, for all purposes, hack saws or rip saws or tenon saws were the best, instead of assessing objectively the uses and limitations of each, we should think the worse of them and their tools. The lapses from objectivity in this book where Spearman is mentioned may give the impression to some of those to whom it is directed that the whole technique is rather suspect—mere 'hamburger-grinding'.

As a sort of powder for those who are upset by this book, the reviewer would recommend as supplementary reading Chapter VIII, pp. 158-170, of Professor Vernon's book *The Measurement of Abilities* (University of London Press, 1940), where a more objective general account of factor analysis may be found.

In discussing the role of factor analysis, the author wisely confined himself to the field of psychological tests. Factor analysis is very helpful in analysing what Stout would have called 'psychoses'—complexes of measures taken at a point of time. Where many reliable tests can be given to many people, factor analysis can be a powerful tool. But in the industrial world such tests and samples are the exception rather than the rule, and the possibilities of factor analysis are limited.

Of more timely service to the statistician in industry would be a method of intercorrelating observations over time, such as indices of production, labour stability, turnover, absence, and quality of work, and extracting the principal factors from them.

DAVID DUNCAN

An Introduction to Mental Measurement and its Applications. By C. A. RICHARDSON. London: Longmans, Green, 1955. Pp. vii + 102. Price 8*s*. 6*d*.

Mr Richardson is well known as the author of some of the earliest and best constructed group intelligence tests in Britain—the Simplex and Simplex Junior. This book provides a brief and quite elementary introduction to mental testing and statistics. It is, perhaps, most suited to education students and teachers who have difficulties in 'getting the hang' of standard scores, reliability, factor analysis, etc. from the ordinary statistical textbooks. But its very brevity involves a good deal of condensation and a somewhat abstract treatment. One doubts, for example, whether many of those for whom the book is intended can really be expected to understand rotation of axes.

The statistical chapters are followed by several short, but very sensible chapters on the nature of intelligence, the use of tests for classification of children, secondary school selection, and other applications. Four pages on vocational guidance hardly seem sufficient to be worth-while, though they do manage to bring out the limitations of tests in this field. There is an interesting point in the chapter on educational selection, namely that one reason for its causing so much heart-burning is that it really acts as an instrument of vocational selection. If employers were more willing to accept leavers from the upper streams of modern schools (who are often as able as the lower streams of grammar schools) many of the problems might disappear. Instead of spending further effort on the impossible task of perfecting selection, we should try to increase educational and vocational opportunities in the modern schools.

P. E. VERNON

The Prediction of Student-Teaching Success from Personality Inventories. By F. T. TYLER. University of California Publications in Education, Vol. XI, No. 4. Berkeley and Los Angeles: University of California Press, 1954. Pp. 233-313. Price $1.25.

School teachers probably constitute the largest and most accessible of high-grade occupational groups. One might have expected, therefore, that psychologists would have been able to work out effective techniques of selecting them. True, there have been hundreds of studies, but the results are so inconsistent and so unimpressive that we have to continue to rely on expressed interests, academic qualifications and personal interview, though the two latter clearly possess very poor validity. Professor Tyler's contribution takes us no further than many previous ones. It shows that very small positive correlations can be obtained between personality questionnaires which measure some kind of mixture of emotional stability and extraversion, and supervisors' ratings of teaching success; but that even when the best differentiating sub-tests or items are selected, they completely fail to stand up to cross-validation in a fresh group. However the investigation is considerably more polished statistically than most of its predecessors, and it provides an interesting comparison of multiple regression, discriminant function, and multiple cutting score techniques. (The last of these—which is far the simplest—seemed the most effective.) The monograph also contains a valuable critique of the defects of earlier studies. One point apparently not mentioned is that teaching success is, in all probability, a far from homogeneous or unidimensional variable. It seems extremely likely that all sorts of different personalities are capable of becoming good teachers (or bad ones). Hence psychologists should not expect to make much progress in measuring an ideal type of teaching personality. The situation is not dissimilar to that in Army officer selection. There too no combination of linear variables appears to have any worth-while validity. But the more flexible, even if more subjective, approach based on group observation has at least provided something more valid than the interview. If, in the future, the supply of teaching candidates begins to exceed the demand, an extension of WOSB methods (along the lines that are being tried out at Manchester) seems the most hopeful means of selecting better teachers.

P. E. VERNON

OCCUPATIONAL PSYCHOLOGY

OCTOBER 1955 VOLUME 29 NUMBER 4

Fifty Years of Psychology

By F. C. BARTLETT

This paper is based on two lectures given at Oxford early in 1955 by Sir Frederic Bartlett, C.B.E., F.R.S., formerly Professor of Experimental Psychology in the University of Cambridge.

I: THE LAST FIFTY YEARS

AT the beginning of the present century, psychology, considered as a scientific discipline, was in a somewhat curious and shaky position. In Germany it was reasonably well established as a proper subject for study at University level, and it had been so for about thirty years. Elsewhere, with a few exceptions, it was usually regarded with strong suspicion, and considered either as the sport of rather cranky people or as a serious threat to the dignity of man which, as everybody knows, rose to great and conscious heights during the later part of the 19th Century. By the year 1900 all of the great, pioneering German experimental psychologists were getting old or oldish. Perhaps when anybody grows oldish, especially if he is a psychologist, and more especially if he is a German psychologist, he turns readily towards systems and theories and away from the slogging and maybe more exacting work of the experimental laboratory. At any rate, that is mainly what these outstanding German psychologists had done or were doing. They were regarded in their own country and in every other (where they were regarded with approval at all) with what now seems to be an exaggerated veneration. They powerfully reinforced what is perhaps a natural tendency, for a man to spend a few years of early vigour carefully collecting accredited facts by controlled experiment, about some special field of human or animal behaviour, and then to build up on those facts an enormous superstructure of systematic belief, alleged to cover the whole range of animal and human life and experience, one of the main merits of which will be that it differs from every other such superstructure raised by every other investigator. What they did helped to fasten upon scientific psychology an urge for the all-embracing theory which it has taken fifty particularly strenuous years partially to overcome. The relative liberation which has now been achieved has raised its own difficulties, principally, of course, by accumulating masses of specialised facts which, to a quick view, seem to lack any sort of integration or synthesis.

In one respect the German position in 1900 is singularly interesting to us to-day. The earlier scientific psychologists had been pre-eminently preoccupied in trying to discover facts about the operations of the special senses—of vision, hearing, touch, taste and smell—and of fairly simple bodily movements. The brilliant Ebbinghaus—whose active experimental career was to stop before our century opens, and whose life, alas, terminated after a sudden attack of pneumonia in 1909—published his famous book on human memory in 1885, and this broke into an entirely new field by showing how experiments could be carried out on very much more complicated behaviour than the early psychologists believed possible. Other people began to think of getting into more complex fields still, but there was a lot of opposition from the vested interests which had won a great reputation by working upon a narrower front. There was prolonged delay, but at last the adventurers broke the barrier, and in 1900 the most active experimental group in Germany was one that was boldly working on the psychology of human thinking processes. Interest in their enterprise waxed rapidly and waned almost as rapidly. They wrote a lot of prodigiously long, and often boring, reports in highly technical German. They started quite a lot of other people to work, in America and to some degree in this country, but partly they worked their ideas to death, and partly they were interrupted by the War. The somewhat sceptical generation which followed the First World War saw their efforts, if they saw them at all, very much as we see pretentious great monuments on the top of some hill and call them somebody's 'folly'. But they did have some ideas, which have never been entirely lost sight of, and it does seem to me extremely interesting that the problems which were being most actively pursued in 1900 have lately come to life again, though the methods that are now being proposed are very different, the aims less sweeping, and the approach altogether more empirical. Of this, more later.

Let us switch from Germany to England. At the beginning of the century there was already quite a respectable amount of experimental psychology at Cambridge; very little elsewhere. Dr W. H. R. Rivers had for some years been lecturing on the special senses and had experimented on fatigue and the effects on behaviour of certain drugs. Dr C. S. Myers was already at work on auditory perception and was taking effective steps to build in Cambridge the first English experimental psychology laboratory. Dr Henry Head, the neurologist, was carrying out with Rivers their famous experiment on nerve division. Rivers was the experimenter, Head the sufferer. The experiments were painful. Head used to come to Cambridge for week ends, and they did their experiments in Rivers' room, in the Second Court of St. John's College. The then Master of St. John's, Sir Robert Scott, told me how, on a Sunday morning, he saw a group of college servants beneath the open window of Rivers' room, apparently having a good time. He went to one of them and said, "What's up?" "Well, sir," was the answer, "we do like to 'ear Dr 'Ead swear." However, this lecture mustn't be much about the work of particular people, sacred or profane, but rather about general trends of thinking and experiment.

Looking back now at the years before the beginning of the 1914-18 War it seems that experimental psychology then made reasonably rapid and sure progress, though it appeared slow at the time. Myers had a small but very active group at work, and, also at Cambridge, Sir Michael Foster had got together a brilliant team of physiologists who, being mainly concerned with activities of the peripheral and central nervous systems, were willing to take a greater interest in experimental psychology than many other scientists at that time took. Sir Charles Sherrington, who was one of this group, gave it tremendous support to the end of his long and distinguished life, and his own book on *The Integrative Action of the Nervous System*, published in 1906, did more for physiological psychology than any other single book accomplished before or since. William McDougall, another of the group members, did his early, and far and away his best, experimental work, on vision. University College, London, made a start in a single-room laboratory. McDougall went there for a year and then removed to Oxford where he became rather less experimental and rather more systematic. Edinburgh and Manchester both got going a few years before the war.

Charles Spearman arrived at University College in 1906. He had already published, in America, the famous paper entitled *General Intelligence, Objectively Determined and Measured*, which probably did more than anything else—though there were other influences in the same direction at the same time—to fasten upon experimental psychology two movements which still run vigorously over the whole field; the use of tests, at first for intelligence but then for every mortal aspect of behaviour that ingenious minds can think of, and the development of psychological statistics.

The War came. Academic psychology languished. But experimental psychology moved into new company taking with it, however, its traditional problems. So far as this country went, apart from some inevitable development of test methods for military selection, its main concerns were to use and expand what was known about auditory perception to help in the hunt for enemy submarines, and what was known about vision to aid the swift and effective growth of the Royal Flying Corps.

Beyond question the use of experiments in the study of human behaviour emerged from the war, in this country, with enhanced status. Yet all of its leading exponents still felt that they had to fight for respectability.

This was the period during which Freud had pretty well the whole world, of knowledge and of ignorance, at his feet. Freud had already been at work for a long time. But *The Interpretation of Dreams*, perhaps the most original and influential of all his many publications, appeared in 1900 and a translation into the American language 10 years later. His fame reached its greatest heights during the 1920's. The odd thing is that the scientific psychologists, not only in this country, borrowed the clothes (if that is the right word to use—I doubt if it is) which Freud had provided, in their ardent quest for respectability. And the odder thing still is that it was this borrowed raiment which very largely got them what they wanted.

All of Freud's enthusiastic adherents among the experimental psychologists regarded him as pre-eminently a great scientist. In fact he never, well hardly ever, carried out an experiment in the strict sense of the word, and he made no pretence to use what is often considered to be the indispensable method of the experimental scientist, that of quantitative measurement. His method was the case study; his strength the most complete loyalty to facts as he observed them, an unshakable belief in his interpretation of the facts, a vast power of intuition or insight, and a great sweep of brooding imagination. In fact he raises in a most intriguing form the question of the criteria by which a man is to be called a scientist. It may be that all the great scientists are people who work as artists in a field which everybody considers to be scientific.

Whatever may be the ultimate verdict on the sombre picture Freud painted of human life and thinking, there is no possible doubt that his impact on experimental psychology was terrific, or that its chief effect was to make the whole subject more humane. When you look at a lot of experiments of the early classical period of experimental psychology, and at many of their direct derivatives down to the present day, you cannot escape the feeling that the experiments mainly provided strings for pulling puppets.

Another formative force now appeared upon the scene. I. P. Pavlov had, by experiment, discovered the principle of the 'conditioned reflex' in the late 1890's. He described and developed his work in a series of brilliant publications from 1903 to 1928. It seemed that here was a genuinely objective method by which the learning processes of animals and men, which come into play in all their most characteristic complex behaviour, could be studied and understood. Moreover, it seemed a method that could be used as well outside as inside the laboratory. Speaking only in a metaphorical sense, it appeared that Freud, the extreme subjectivist, and Pavlov, the extreme objectivist, could join hands in putting the real man in the centre of psychological study, though when their views were rushed to extremes, as they very often were, the two could only fall apart again and glare at one another.

Anyway this was a period of ferment for all kinds of psychology throughout Great Britain. It was also the period when that division which invades most branches of science, but is potentially more disastrous to both sides in psychology than anywhere else, threatened to become acute. Those who wished to study and understand human beings, with all their equipment of motives, wishes, beliefs, desires and emotions, tended to move out of the laboratory into the open world and to work at problems called 'applied'. No doubt they often made over-optimistic claims; they used half-baked methods, in particular replacing the rigour of experiment by all manner of ill-accredited diagnostic tests, and altogether they acted like people who were in a desperate hurry to get somewhere without overmuch knowledge of where they were going. It is not surprising that the other people who believed profoundly that the only way to get any genuine understanding of animal behaviour is the way of the closely controlled experiment were rather resentful, rather stand-offish, rather 'superior', as we say. Nevertheless it has now become plain that this was a mistake.

However, in one direction or another, scientific psychology in Great Britain was now fairly on the march. By the time of the Second World War practically every University had its established psychology department, and most of these were experimental. Myers had his National Institute of Industrial Psychology going strong, and there was increasing interest and support from bodies like the Medical Research Council and the Royal Society.

The overall effects of the Second World War on experimental psychology were so nearly the same in the U.S.A. and this country that now is the time to make the switch to America.

I must disclaim any attempt to give an adequate account of American developments as a part of a single lecture. It is wildly impossible. For any who may be interested there is a lively, rather unconventional book by Dr A. A. Roback, called a *History of American Psychology*. The book has been heavily manhandled by certain other American psychologists; but it is pretty good. It is significant that when he comes to the modern period, though he is writing at considerable length, he finds that his only chance of reasonable exposition is to write of 'The Schools'. In many ways Americans soon moved miles away from the Germans who had started them going. They were more open-minded, more curious, more exploratory, perhaps above everything else more pragmatic, judging the value of a method by the results which it appeared able to achieve. But they also ran, they even galloped, towards the all-embracing theory. Far more ready than we were to recognise without limit the claims of experimental method, they were, at least up to the end of the 30's, and probably they are still, much less empirical than we are. The Schools of which Roback writes are Structuralism; Action Theory and Applied Psychology; Functionalism; Behaviourism; Dynamic and Hormic Psychology; the impact of Freud; Gestalt Psychology, Operationism: Factorial Analysis; Topographical Psychology. When he comes to the contemporary scene even Roback's enthusiasm balks, and his history becomes little but a very long catalogue of names.

America was vastly quicker off the mark than we were. By 1918 there were strong experimental psychological departments all over the place, in Universities and Colleges, and there was lively public interest. Very soon after America joined in the war, large numbers of American psychologists were engaged in the first large scale trial of psychological tests for the mass production of military officers. The U.S.A. came out of this war, as we did, far more willing to recognise and use experimental psychology, and the backing of tremendous public enthusiasm which they got was no doubt a bit of an embarrassment. It was, however, during the next fifteen years or so that America unmistakably took the lead in scientific psychology in the whole world, probably for good and all. Certainly this was the case as regards quantity of work done and general internal status achieved. In quality of work it was equally certainly the case in several departments of the subject, particularly in the experimental study of animal behaviour, in sensory psychology, especially in psycho-acoustics; probably in psychological statistics, in an ardent pursuit of all manner of methods that seemed to promise even a faint hope of experimental verification for the study of social problems, and in that improvement of instrumentation which is inseparable from experimental advance.

So we reach the Second World War. The general development of psychology in the other countries of Europe had been very much the same as in this country, except that it was slower and there were differences of emphasis. The second war pretty well crushed psychology in the whole of Europe outside England and Scotland, and only now is recovery getting well under way. In Great Britain and America the results were radical and, in a broad way, identical.

The overriding difference between the two wars, from our point of view, was that in the interval the Fighting Services had become highly mechanised, and this process went on at a constantly increasing rate once the war began. For many reasons and in many directions the use of selection tests could not deal adequately with the demands. It became necessary to consider the design of machines in such a way that they could be effectively operated by the great bulk of the members of any normal random population. This can be done only if knowledge is available about the normal ranges of capacity, in whatever directions may be required, of men and women. It also calls, even more obviously, for physical, engineering, and often mathematical knowledge. For the first time in history psychologists had to work closely with physicists, engineers, and often with those expert in applied mathematics. Each group learned to understand a little about the problems of the others, and in general to have increased respect for their various lines of approach. Especially as electronics developed and more or less self-regulating instruments came to be used, psychologists and physicists alike began to consider in new ways the possibility of producing models for human behaviour. The problems of how best to supply information to instruments of the computer type, which would then store it, and use it when any occasion arose within the programme of the machine, seemed to have tremendous implications for the understanding of all processes of human memory. The operations of several such instruments in chain seemed to have equal implications in connection with the human use of language communication. The studies later popularised as *cybernetics* had an independent beginning in this country with the work of Kenneth Craik. Communication and information theory, which exercises an enormous influence on current work in psychology everywhere, were more specifically an American contribution.

But another thing happened, more important still in my opinion. All this concentration upon behaviour with machines forced psychologists to make a major problem of the study of *skill*. Every example of skilled behaviour involves a sequence of steps, or movements, bodily or mental or both, in such a way that every movement after the first possesses properties which depend, not only upon the immediate external stimulation, but also upon its timing and other relationships to preceding and succeeding movements. Nothing fundamental can be discovered or understood about skill by a study of the direct results of environmental stimulation in the production of what can be treated as single response items. Consequently experimental psychologists had to search for new methods of recording and measuring complex behaviour. There is plenty left still to find out, but the search has been successful, and this has meant what amounts to a basic change of outlook as contrasted with that which was common at the beginning of this century. In every live experimental psychological laboratory and research unit in the world now, the situations that are being studied are more realistic, more closely allied with everyday behaviour than in the earlier period. The old clash, the old enmity between what used to be called 'applied' and 'basic' or 'fundamental' investigation is in rapid process of disappearance. Also the emergence of methods for recording and measuring complex human behaviour, especially where it is combined with that approach, due largely to physical and mathematical influences, called Theory of Information, have brought again into the field those very problems of how to understand, and use, the higher mental processes, and in particular those of human thinking which, as we saw, were also prominent at the beginning of the century. Then, though they were given an experimental form, their study was essentially speculative. Now it is coming to be genuinely experimental and empirical.

If we look back at the last fifty years of experimental psychology we can, in a broad way, distinguish three main phases or periods of development. To begin with, the view was that animal and human behaviour, however complex it appears, is only a compound of extremely simple elements, all sensory in nature and all directly and uniformly determined by environmental stimulation. The man became a puppet, activated by strings, and every time a particular string, or any combination of strings is pulled, exactly the same result follows. But animals are not like that, because as soon as any behaviour response is set up it has to be continued not only on a basis of the outside stimuli, or the strings, but on that of the further clues which come from inside the responses themselves.

In the second phase there came a kind of romantic revolt, led in a clinical sense by Freud, and in an experimental by the group who called themselves *Gestalt* psychologists.

Today there is a return to the more classical, the more analytic approach. But it is not just a swing back of the pendulum. The model is not the puppet, but the self-regulating machine. It is at last realised that human behaviour may be stable, though there is no need to regard the elements into which it can be analysed as themselves fixed and stabilised, and without any range of freedom. Now pretty well the whole field of animal behaviour is opening up to the experimentalist. It is true that he will often need also that kind of critical clinical judgment and power of prognosis which are needed in medicine. To the adventurous mind however the opportunities ready for development are greater than they have ever been before.

2: THE POSITION TODAY

I have said that anybody who takes a quick glance at present-day psychology is likely to be bewildered by its variety, and by the manner in which, often apparently without much consideration, psychological pronouncements are made about all kinds of desperately difficult problems. But a more extended and careful study will, I am sure, provide convincing evidence that all this excitement, this spirit of adventure that is abroad in psychology today, this apparent scurrying hither and thither, are the outcome of a few formative and fundamental influences, and are an attempt to reach a few issues all of which are closely related, and all of which are moving towards clearer definition. I shall now attempt to say what, in my opinion, these basic influences and these aims and issues are. I do not suppose that I can reach a formulation which will commend itself to everybody, but honestly to try to do something of this sort seems now to me to be an inescapable charge upon everybody who is interested in the scientific study of man.

The one thing we must keep in mind the whole time is that the most active developments in current psychology are the direct result of movements that were set up during two wars and two restless and critical post-war periods. The overall effects of the first war and the first post-war period were exceedingly different from those of the second. It is hardly too much to say that psychologists all over the world, whether they belonged to the belligerent groups or not, emerged from the First World War chanting in unaccustomed harmony the words of the old jingle

> "God has a plan for every man
> And He has one for you."

There was a rather naive and optimistic belief that every man was, by nature or training or both, designed to be a specialist, and that this design could be unambiguously demonstrated. It is interesting that this belief, which was carried to great extremes in America, but was pretty firmly established everywhere else as well, should have led to an enthusiastic boosting of tests, and of the statistical analysis of their results, at the expense of experiment proper. For it can best be understood as one of the expressions of a sort of romantic revolt due mainly to the predominant influence of Freud in a society which was everywhere very much against all forms of alleged rational and established control. Tests must always be more concerned with behaviour directly, and even exclusively, rather than with attempts to establish the conditions or causes of behaviour. Moreover psychological experiment has always been, and still is, a little unhappy when it is confronted by a demand to produce controlled situations in which the leading influences are even the superficial emotions. Once it comes to be believed that every form taken by behaviour is largely due to deep-lying and, in an ordinary sense, inaccessible emotions, it is not surprising that tests tend to get the upper hand.

Nobody can as yet be quite sure what position tests may ultimately come to have in a science of psychology. In many cases they may perhaps be seen as an indispensable preliminary to experiment. But, looking at present-day psychology, it does seem fair to say that the first war and the first post-war period made no very vital or radical contribution to experimental methods and problems. What they did unquestionably do was widely to popularise a psychological approach to any and every problem involving animal conduct, and to familiarise people everywhere with the general notions and, to some degree, the terminologies of the contemporary psychologists. They also did much to shake professional psychologists free from an exaggerated respect for their own ancestors. They produced, in fact, a situation ripe for the far more radical changes which the second war and the second post-war period were going to produce.

To these we will now turn.

By long odds the most potent influence shaping the problems, the methods and the general outlook of psychology today was the enormously rapid development of technological invention. This, already under way, of course, before the war began, naturally got a tremendous push when the war broke out, first in the air and on and under the sea, and then in the control of nearly all the weapons of land based forces, whether for attack or for defence. As everybody knows, it has continued ever since with accelerating speed and with ever-increasing expansion into practically every field of human action. Speaking broadly and in reference to psychology the influences of this astounding development of machine invention are to be seen in two main directions. First psychologists have been induced to give a great amount more of attention than ever before to those streams or sequences of individual behaviour the course of which is technologically controlled. Secondly the growth of new methods and speeds of communication has raised in far more accessible forms than before problems of the interplay of small and large social groups.

Take first what is happening about the study of individual behaviour.

It might well seem that technological development is directed to produce a world of specialists. In a sense this is true, but if it were all of the truth it might seem as if what ought to have happened for psychology is a very heavy reinforcement for all those test methods that are directed towards the discovery of specific capabilities. Almost the exact reverse of this has in fact happened and for this there are two main reasons. The speed of invention is such that no sooner has any particular activity become controlled by machines than we have to begin to contemplate modified or new forms of control. More important still, while technical inventions dictate the nature and sequence of the skilled activities which have to deal with them, they do not as a rule affect the environment in which such activities have to be exercised. In fact other inventions, mainly by increasing the chances of mobility in all sorts of directions, have raised many new problems of environmental stress. It is no accident that, just when the whole world and psychology itself is apparently moving quickly towards a phase of extreme specialisation, the view of human behaviour as the expression of a highly fluid and adaptive mechanism is gaining ground everywhere. Tests may tell us what a person does or, within limits, can do. But to understand adaptive behaviour, to determine its limits of range, we must know also *how* the operations concerned are carried out, and only experiments can tell us much about that.

The next point is, I think, more important still for a proper understanding of the present position. Almost all technologically or machine-controlled behaviour moves in a sequence of events. Sequences were never very much of a concern in what may be called classical psychology. On the whole the guiding principle was that of stimulus-response. Often enough, it is true, 'stimulus' has been so defined that it had to be considered as a collocation or organisation of stimuli acting in a unitary manner. But this makes no real difference to the basic principle: action is still regarded as entirely, or almost entirely, the outcome of conditions that lie outside the action itself. As soon as ever it is realised in a practical way that by far the greatest part, and all the most important part, of human behaviour in the world as we have to deal with it, is skilled behaviour, the 'lie' of psychological problems is radically altered.

During the last eight years I have had the opportunity to make three fairly extended visits to the United States of America. As I have already said, we must admit that North America has, in a number of directions, moved into the van of psychological development. The position there at present is intensely interesting. Anybody who confined his attention to certain departments, those concerned with routine academic teaching and research, in the well known and longest established Universities, might well be tempted to think that very little genuine change has been introduced over the last 20 or 30 years. There is still much experimentation on animal learning and behaviour of a kind which, apart from greatly improved instrumentation and a tremendous lot of talk about motivation—not, in my opinion, very effective talk—might almost as well have been planned in the early 1900's. In a few places there is outstanding work on sensory processes, all still in the classical line of approach, but, again, with enormously improved instrumentation and with a very strong tendency to seek explanations phrased more and more in physiological, neurological or biophysical terms. But everywhere there are younger groups whose driving ideas come, for the greater part, from research units that are scattered up and down the country, many of them with no direct University affiliation at all, but all of them centres in which engineering, physical, mathematical and biological interests are combined. Their predominant interest is the attempt to find experimentally demonstrable measures of skill at various levels—of bodily performance, of perceiving, of remembering and of still more complex mental functions—which will define their nature and the limits of their range. Many are investigating the environmental conditions of which I spoke just now, especially particular forms of stress—of climate, of speed and of load—always in terms, not of single or incidental performances judged in terms of their output, but of continued performances in which influences play forth and back from step to step. The measures that are being developed are not much, certainly not alone, in terms of a final issue, or of output, but in terms of the internal relationships, of timing, of number, of order and of direction of the steps. All animal behaviour is seen, both as specifically directed, and as readily and widely adaptable.

It may be—I think it will be—that this twist towards the experimental study of skilled sequences in performance, will turn out to be by far the most lasting and radical movement in present-day psychology. Already it is achieving new methods, and new ways of assessing their results, not least a statistic capable of dealing adequately with trends of direction.

It has certain theoretic consequences however about which it is not yet possible to be anything like as certain, though they are attracting an immense amount of attention at the moment. Perhaps the most outstanding character of skilled response is its claim to be objectively determined. We call an action unskilled either when some of its steps in sequence overlook or misinterpret the environmental demands, or, by a different sort of criterion, when the demands are of so general a nature that they do not need to be very closely followed. So it is possible to regard all skilled behaviour as dominated by the reception, interpretation, storage and, in many cases, anticipatory use of information.

Also this is a period in which we are seeing the extremely rapid development of machines, the purpose of which is to automatise large areas of skilled behaviour, and so to set the human operator free to work more swiftly and with more sources of information at one and the same time. All such machines are of the self-regulating type and so they work, like the human operator, by accepting information supplied from the environment, if necessary storing it, and adjusting their response to the demands of the moment. Two extremely active movements in current psychology have followed from this. The first is to take these electronic devices as possible models of how the central nervous system copes with information supplied, and the second is to study the forms in which information can be most effectively supplied to these machines, and the orders of classification of items of information which will best produce automatically the desired (or programmed) response.

When I went to America seven years ago practically all the active psychologists were talking enthusiastically about the first of these movements, to which Norbert Wiener had given the popular name 'cybernetics'. Last year very few people were giving it much attention except to use terms like 'feed back' and 'scanning' in a general, speculative and dogmatic way. So far the movement does not seem to have done very much except to add a few useful words to the psychologist's vocabulary. But it is very likely to revive. Everywhere, on the other hand, exceedingly lively psychological minds were speaking and thinking in terms of Theories of Information and Communication. These are mostly physical and mathematical in origin and they certainly cannot be clearly described in a part of a single lecture, even if it is possible in a psychological sense to make them clear at all. Perhaps it is fair to say that they are concerned chiefly with the rate, range and form in which information can be supplied, whether to machines or to man, and stored by them, so as to give the greatest probability of its unambiguous and successful use. I may add, tentatively, that at present it seems as if the information and classifications of information have to be regarded as operating within 'closed' systems so that items and combinations of items lead only to a finite and calculable number of alternative interpretations and related actions. Whether this approach can ever match the vast diversity of life and action seems uncertain. But it is at least certain that development along such lines is leading to new and fruitful experiments.

Both of these movements land us plumb in the middle of an old controversy which, in its modern form, is agitating all psychologists today. The aim of the machine is extreme accuracy. Perhaps the primary demands upon the human mechanism are for extreme adaptability. Can behaviour achieve both? If it can, does it mean that we have to think of a type of neurological, or mental, organisation that has developed along lines fundamentally different from those of contemporary self-regulating machines? Nobody yet knows the answer to these questions, but they are very lively issues. Meanwhile the whole study of human cognitive processes, especially in their high level expressions, is undergoing a very thorough re-orientation. All of these are now seen, not as single responses to groups of stimuli but as taking place through a number of closely inter-related steps whose number, order and direction may be relatively closely determined. Many people, especially those whose main approach is physical, mathematical and engineering, see thinking, for example, as a continued development of conditional probabilities. What may be the upshot of all this ferment nobody can tell, but equally nobody can doubt that these raise problems about the higher mental processes which, after languishing for many years, have come alive again.

There is nothing like enough time left to consider properly the current resurgence of interest, especially in America, in social psychological problems. This is certainly due in part to the emergence into the field of psychological discussion of those Theories of Communication which are the direct result of the efforts of the electronic engineer to deal with practical communication problems during the second war and the post-war period. More broadly it is due to the mass of technological developments which have enormously increased what may be called psychological mobility, and put all social groups everywhere, large and small, into effective contact one with another. Everybody now sees the bulk of human behaviour as, partially or wholly, directly socially determined. There is a somewhat feverish rush around to exploit any and every method that seems to offer even a remote chance of throwing light upon how this social determination operates. Perhaps it is not unfair to distinguish four main lines of psychological approach.

The first sees all social influence as operating through individual attitudes and sets to work to try to find out what are the fundamental socially fixed attitudes and their distribution in selected groups or communities. The characteristic methods are the use of questionnaires, social inventories, interviews, clinical examination and 'tests' of almost every imaginable kind. The results are usually analysed statistically, very often by factorial methods, in an attempt to establish a small number of fundamental and ineducible social attitudes and then, usually by correlational methods, to arrive at typical attitude structures widely distributed in whatever groups are being stated. Apart from many difficulties inherent in the method, the main trouble seems to be that all this can show nothing about the impact of attitude upon opinion or the impact of opinion upon practice.

The second method frankly demands the acceptance of some kind of systematic and rational statement of leading effective social influences. The ways in which these work are illustrated in a survey, sometimes of social groupings in general, more often nowadays in one or more selected groups throughout particular periods of their history. There is, of course, nothing new about this approach, but it is characteristic of the times that it should be used mainly in reference to contemporary events and that the analysis it effects should be alleged to be based upon direct psychological observation rather than upon a speculation. This is the method used in innumerable studies of propaganda, of the cinema, television, the popular press and the like. An excellent example is a recent book called *The Dynamics of Democratic Government* by Rooke and Stedman, two psychologists from Cornell and Columbia Universities. The difficulty of this approach is that it demands the uncritical acceptance of whatever psychological theories form its foundation.

The third approach seemed likely a few years ago to become both popular and effective, but it has not lived up to its promise. It attempted to do direct psychological experiments with small social groups. The difficulties were that as it was tried it always demanded the acceptance of some underlying and unproved general psychological theory, and, far more important, that all the really effective groups in the modern world appear to be large, and all the time getting larger.

Finally, there is the attempt, generally based either on Theory of Communication, or upon established results of psychological experiment, to form hypotheses about the ways in which social influences may be expected to influence particular functions, like perceiving, thinking, and motivation. This is the line of the Princeton attempt to build up a social psychology on a study of those highly complex situational perceptions suggested by Ames; of the Harvard attempt to show the effects of socially derived values also upon perceiving; and of many attempts to open up afresh the problems of thinking. It is a very long term approach. It has many difficulties, the chief of which is that very often the experiments seem to carry their results in their design; but in the long run it may well turn out to be the most effective of all these four movements.

This attempt to deal briefly with the position in psychology today has inevitably been very highly selective. There are many aspects of current investigation of which I have purposely said nothing at all. Other psychologists would make different selections. What I have tried to do is to pick out those developments which represent something really new. Broadly they are these:

(1) The experimental and observational situations which are now being studied are vastly more realistic than they have ever been before. This is not just because psychology is attempting to answer practical problems in the sense of being immediately useful, though there is a good deal of that. It is because all human behaviour is seen as a more or less continuous adjustment to the manifold and continuous demands of the outside world. The main result is the development of new methods of experi-

ment and measurement based upon the inter-relation of active processes rather than solely upon their final issue or output.

(2) This re-orientation is due in the main to psychology's forced alliance with technological invention, and to guiding ideas which, also in the main, came from engineers, physicists and mathematicians.

(3) The great over-riding problem now is how to reconcile demands for accuracy in behaviour with demands for fluent adaptability, and whether these two fall into genuinely incompatible categories. The older psychology, on the whole, either went to undue lengths to get rid of variability from behaviour, or, recognising it, to land the whole subject in a welter of assertions about individual differences. Perhaps it may be possible to determine degrees of freedom within which the constituent items of complex behaviour are expressed, and so to understand and where necessary to control behaviour, without reducing it to repetitive routine, but still treating it as lawful and orderly.

(4) There is a keener recognition of the importance of genuinely social determinants of behaviour, and although it cannot be said that there are as yet any widely agreed methods for their controlled study, the prospect, at least, is set fair.

Psychology today is alive, alert, explorative. It is better able than at any other time to stand up to the rigorous demands of the longer established sciences without relapsing into artificiality. I believe it to be in tune for significant advance.

A Neglected Factor in Labour Turnover

By ROBERT H. GUEST

The investigation reported here is one of a series of studies being undertaken currently by the Institute of Human Relations, Yale University. These studies are directed by Charles R. Walker, Director of Technology and Human Relations for the Institute's Technology Project. Mr Guest is in charge of field research for the Project.

INTRODUCTION

THE classical economic theories of 'labour market' which hold that wages are primary determinants of the movement of labour have been seriously questioned in recent years by students of the behavioural sciences, by men of industry and labour, and by the economists themselves. Reynolds and Shister (1949), following a study of worker behaviour, observed:

> "Economic theories of the labor market clearly need to be reconsidered in the light of actual worker behavior. . . Such sophisticated concepts as a 'labor supply curve'. . . at once exalt wages to a position of undue prominence and imply greater precision in workers' choices than actually exists."

The authors then suggest that further efforts be made to probe more deeply into reasons why men remain at work or seek employment elsewhere. The problem has received much attention from social scientists and men of industry in the form of enquiries into the nature of job satisfaction. The immediate work situation apart from the economic rewards has become a focus of study. Beginning with the Mayo studies, and later investigations by followers of Lewin, the Michigan group, and many others, the enquiries attach considerable importance to the social factors in the work situation, a man's relationship to his fellow workers, to supervision, and to the formal organisation of union and management.

Considerably less emphasis has been given to the intrinsic nature of the work itself and the way in which attitudes are affected by man's relationship to machines; i.e., to the technological imperatives of the job. The British have perhaps contributed more to this area of enquiry than anyone else.

The study reported here, while it supports what has been said about the importance of socio-economic factors, underscores the influence of the non-social or technical factors related to the immediate job. It is a study of a small number of workers who quit their jobs after working several years in an automobile assembly plant in an Eastern metropolitan community of the United States.

BACKGROUNDS OF THE STUDY

The origins of the present study began as a result of an early enquiry (Walker and Guest, 1952) into the relationship between mass production and job satisfaction. In this enquiry covering 180 men, it was found that, in spite of a relatively high wage rate, a primary source of job dissatisfaction was in the nature of the work itself, the mechanical pacing of the line, and the highly repetitive motions.

The early study covered production workers who had been engaged in auto assembly line work for less than two years. A second study was then carried out at another plant, considerably older, to determine whether men of long service became adjusted to this type of work. In general it was found that the same job characteristics were of primary importance in explaining job dissatisfaction and particularly so because of physical problems facing men approaching middle age. About half of these workers said that if the right opportunity presented itself, they would quit their jobs on the assembly line.

During this second study* a number of men did quit, and an attempt was made to discover why they left and where they went. Personnel information was examined for eighteen of these men. Extended interviews were then made with this group in their homes. A schedule of over fifty questions was drawn up pertaining to the several aspects of ‘ life on the job’, both in their present employment and in their previous employment on the assembly line. The age median for the group was thirty-six years and the age range was thirty-two to forty-eight.

COMPARATIVE FACTS ABOUT FORMER AND PRESENT JOBS

Those studied were all former production workers with twelve to fifteen years’ seniority. Wages were equal to, if not slightly higher than, wages in the general industrial area. Most of the men had held jobs in which the work pace was determined directly or indirectly by the moving conveyor. Although the immediate content of each job varied from one to another in terms of tools used and materials, the characteristics of the job were generally similar. Mass production principles translated into experience from the workers’ point of view meant (Walker and Guest, 1952):

(a) Mechanically controlled work pace
(b) Repetition of simple motions
(c) Minimum of skill
(d) Predetermination of tools and method of performing operations
(e) Minute sub-division of the product
(f) Surface mental attention

To these may be added the fact that the work required a considerable amount of sustained physical activity.

> A ‘line’ job is described here by one of the workers: “My job was on the final chassis line where the assembled grilles and fenders were taken off a hoist and lowered on the body and chassis. I did three or four operations. First I made up stock off the line. There were about

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* The particular plant under observation had undergone a long and troubled labour relations history, not typical of other plants in the same corporation. Nevertheless, the basic elements of immediate job dissatisfaction were found present in other plants.

> fifteen kinds of washers and lock washers which were placed in little bins. As the job came down I took the right kind and number of washers and attached them to the front end assembly. They get put on loose. Then my buddy and I grabbed the whole front end assembly as it hung from the hoist and lowered it down onto the chassis. Then I pushed it and pulled it into place and made sure the whole assembly was lined up with the door. It took quite a lot of muscle, but it had to be done right."

Certain striking facts immediately stood out with respect to the workers' new jobs, which they had taken after quitting.

(1) The great majority of the group were getting less pay on their new jobs as contrasted with wages on their former assembly line jobs. Only five of the eighteen said they were getting more money and a close examination of these jobs indicates that the hourly rate for two of these five was probably less.

(2) The intrinsic nature of the new job itself in terms of physical and mental requirements was considerably different from the kind of work performed in the assembly plant. This may be indicated briefly by the job titles:

1. Part owner, gas station
2. Milk route operator
3. Delivery truck driver
4. Bartender
5. State motor vehicle inspector
6. Carpenter and painter, construction
7. Co-owner, used car lot
8. Co-owner, body shop
9. Railroad hose coupler and repairman
10. Furnace service man
11. Platform man, dairy
12. Machinist, ordinance plant
13. Maintenance man, housing authority
14. Machinist, small job shop
15. Cabinet maker
16. Paint equipment sales and service
17. Oil company packer
18. Oil tanks service man

(3) Only three men went into factory work, and these jobs were skilled. No jobs were highly repetitive or conveyer paced.

(4) More than half of the group (eleven out of eighteen) worked full time or part time out of doors.

(5) Only three took jobs in large private corporations.

(6) Experience on the previous job was directly useable in the new job for only one man and of partial use for two others.

Other facts, drawn from personnel records and interviews, were also noted. For instance, half of the group had no dependants other than a wife. None had more than two children. In the study of 202 workers who had remained on the assembly line job, the majority had more dependants on an average.

Were those who quit below average workers? Not in the opinion of management. The last foreman ratings on the assembly line jobs indicate ratings equal to, if not slightly higher, than the average of those who remained on their old jobs.

The several facts given so far throw some light on, but do not explain, the underlying reasons why these men, with a substantial backlog of twelve to fifteen years' seniority, decided to leave relatively high-paying jobs to take a chance on the outside. These were not men who were used to jumping from job to job. They were not 'transients'. They were men who had given much thought to this serious decision. Fifteen of the eighteen had thought it over months and even years before they severed the cord of seniority. We found that fifty per cent of the 202 who remained on their assembly line jobs had also given much thought to quitting, but, for reasons more fully described later, could not take the final step.

A critical difference between the two groups may revolve around the fact that almost none of those who stayed on the job had the immediate opportunity to step into a job elsewhere; whereas, all but two of those who quit had a job lined up and waiting.

We turn now to the men themselves and hear in their own words what they had to say about their present and former jobs and the reasons for leaving. The quoted material with comments by the observer is arranged in the following order: The emotional reaction about quitting; immediate job characteristics compared; ageing and physical fatigue; the cutback; social factors (relationship to fellow workers, relationship to supervision, influence of workers' wives); why men stayed (the dilemma of security and independence).

THE EMOTIONAL REACTION ABOUT QUITTING FORMER JOB

Workers were encouraged to talk freely about the way they now felt, having left their long former employment. Here are the kinds of comments they made:

> *Oil Company Packer*: "I look on my new job as kind of a release. My future is brighter now. I'm relaxed—not nervous any more—no more hustle and bustle. I really am glad I'm out of there. I like a job where I'm not bothered and where I can work by myself. That's why I liked one job that I had at Plant Y working on a bench off the main line. It was a pleasure and the day went fast. On the line you knock yourself out."
>
> *Delivery Truck Driver*: "I sometimes see my old friends at Plant Y when I deliver stuff. They say, 'We thought you were on a leave of absence.' I say, 'Yes, I am—for good.' All my friends are glad for my sake that I got out. That's the way I used to feel when someone left. You'd be sorry the guy left because you'd miss him, but you'd envy him, too, for getting out of that place. I used to think that working at Plant Y was a great thing—you know, building automobiles—but then I got so I would dread going into that place in the morning, I hated it so much, and that's no good. On the job I have now I can set my own pace. Now it seems I feel more like doing things in the evening than before."
>
> *State Motor Vehicle Inspector*: "The job I have now is important to me and to people I inspect. When you think about it, the whole purpose of my job at motor vehicle inspection is to make the cars safe for the highway. No one will ever know how many lives get saved just by me not passing a car and making him align the wheels before he gets his inspection tag.
>
> "I take a lot more interest in reading things now. I subscribe to these two mechanics magazines. If I wasn't interested in my job, I wouldn't do that. At Plant Y I wasn't interested in reading nothing. I have seen a lot more of my friends lately. The other day I joined a bowling league."
>
> At this point his wife spoke up, "You saw him last December when he worked at Plant Y. What do you think about him? He's a different man. He

doesn't look tired. He doesn't come home all fagged out. We go out at night—go to the movies, see our friends. I'm a different person now because he is."

Machinist, Ordinance Plant: "On that old job you do the same thing all the time. After a while you stop thinking. It wasn't interesting. It was just living. This job I have now breaks up the monotony and gives you a chance to think again."

Cabinet Maker: "I like the job because it's different. The company puts responsibility on you to do the whole job. I have to use my head. On the Plant Y job you get so used to the job that it works automatically. You worked like a robot."

These and other comments of all eighteen men make it clear that the change of job had a profound and positive effect on their entire emotional outlook. It is quite possible that over time new dissatisfactions on their present jobs will develop. But it appears unlikely because of factors intrinsic in the work itself that these men will ever want to return to an assembly line.

IMMEDIATE JOB CHARACTERISTICS: A COMPARISON

The quotations above give a hint as to what it was about the former jobs which the men disliked intensely. Workers were encouraged to spell out in more detail just what it was which made a difference between the characteristics of the former and present jobs.

Maintenance Man, Housing Authority: "On my old job you repeated the same thing day in and out—and even there I had more variety of parts to assemble than most men had. Now, everything is different. I do a little of everything, repairing, plastering, painting, mason work, and fixing broken pipes. I think it's much better to change around. I like to change because I learn different things I never knew before. On the old job I was learning nothing. It gets monotonous."

Furnace Service Man: "On the old job I put the same parts in, day in, day out, year in, year out. You get so that you can do it in your sleep. On the line you have to keep up the pace all day. It gets tiresome after a while. On this job now, it's always different. I don't know what's coming up tomorrow."

Delivery Truck Driver: "I used to install windshield moulding. It was the same thing on every car. I wanted to get a job off the line so that I wouldn't have the aggravation. You could get relief only when they wanted to give it to you, not when you needed it. They figured that if I got off because of my ulcer, then all the men on the line with ulcers would want to get off—and there just weren't that many other kinds of jobs. The thing I like about my present job is that I can set my own pace."

Car Inspector: "On my Plant Y job, I was a utility man and so I was on quite a few jobs over a few weeks' time, but when I was on any particular job, it was the same thing over and over again. I just did it, that's all. On this job I have now you wouldn't think there was much to it, but every car that comes along has something different. I always did like automobiles, but I'm getting to know more about what makes a car tick than I did in fifteen years at the automobile plant."

Carpenter, Painter—Construction: "I had a repair job on the assembly line and it was better than most jobs—but you still had to keep up to the line. The thing I didn't like was that the line was too fast and you never quite did the job the proper way it should have been done. I don't mind working fast, but I like to work right.

"What I like about this (present) job is that we get the time we need to finish a job right. Also, there is something different every day. It breaks up the monotony."

> *Co-owner, Car Lot*: "That old job wasn't interesting at all. You were a number and not a human being. I had been thinking about getting out for a long time, about five years. I was never at ease—always under pressure. No matter what you did, you never did enough. On this job now, I work hard, put in a lot more hours than I did before—but I like it. Guess maybe it's because it's my own business, and I want to get ahead."
>
> *Railroad Hose Coupler*: "On that line job they press the button to go and you just have to keep up with it. Production is production no matter how you look at it. All high speed. It's like a race against the line and you can't beat the line. On the job I have now, there is no 'hurry-up here and hurry-up there.' You work at a normal pace. On this job I have, you get more chance to get around—to do different things. One week I am oiling trains, next week repairing tracks, next time hose couplings, next cleaning cars. Variety is the spice of life. Makes time go faster. Why be a mechanical man? How long can you last? You burn yourself out. When things start to speed up and you can't slow down, it's time to leave."
>
> *Oil Tank Service Man*: "The thing about this job I have is that there is so much to learn: I'll always have the feeling I'm getting ahead. On the line job I had, you had to stay there and that's all. How long can a man stand on the line? Here, nobody pushes you, but you go."
>
> *Machinist*: "On the Plant Y job I took windshield glass out of a case, put it on a jig, and wrapped rubber around it. I did about thirty-one an hour. It wasn't interesting. It was just a living. On the job I have now, I machine small instrument parts working from blueprints. Every job has different jigs and fixtures. Doing different things breaks up the monotony and gives you a chance to think again. When you do the same thing all the time, you stop thinking."

These comments, which are typical for all the respondents, underscore the importance of the immediate job itself. The question could be asked: Are there not personality characteristics among these particular men which makes them naturally revolt against repetitive, conveyor-paced work? Or, do not most factory workers prefer jobs which can be performed automatically without the drudgery of thinking? Our evidence from interviews with 180 workers at assembly Plant X and 202 at Plant Y uncovered very few persons who preferred these job characteristics. Those who remained on the job listed these same characteristics as primary reasons for *wanting* to leave.

Let us look closely for a moment again at the new jobs taken by the men who quit to see how the qualitative comments made by the men reflect basic differences in the immediate job content.

(1) On the new job the minute-to-minute activity throughout the work day never repeated itself. On the assembly line each job had an operational job cycle ranging from one to two minutes. Upon completion of a cycle, the same motions were repeated again.

(2) The pace at which one worked on the new job was determined either entirely by the man himself (for six men), by a general schedule set up by the day or week (for five men), or according to the varying amount of work prescribed by supervision (for the rest). On the assembly line the pace was strictly determined by the movement of the conveyor and the work standards set for the individual jobs.

(3) On most of the new jobs the men worked on the 'whole' product or performed the 'whole' of a service. The cabinet maker, for example, put the entire unit together. The gas station owner started and completed each

service operation. The machinist read the blueprints, set up the job, and finished the piece. The maintenance men performed all the operations on a particular repair job. On the assembly line each job was performed on a small fraction of the product. Many men after years of service had almost never seen the completed product roll off the final line.

(4) The manner in which work was to be performed on the new job was not, in most instances, determined by a rigid set of performance standards and specifications. On the assembly line each operational method was closely prescribed.

To one who has not personally experienced manual labour, these differences might not appear significant. Why should assembling automobile parts be so sharply different from delivering milk, pumping gasolene, repairing pipes, or hauling dairy cartons? The jobs after all are essentially physical in character and require few skills.

The men whom we interviewed answered the question. To them the differences were very striking. The new jobs involved more than a simple change from one work environment to another. The jobs took on completely different dimensions in time and space and in degree of personal control. They were not, in short, highly circumscribed by the imperatives of technology itself. That all of these men, living in a large industrial section of the United States, consciously and deliberately avoided highly rationalised mass production work on their new jobs can hardly be explained by chance.

AGEING AND PHYSICAL FATIGUE

Dissatisfactions stemming from the immediate job experience were further intensified by problems of ageing and physical fatigue. As workers approached middle age certain constitutional ailments related to age had begun to appear. The physical requirements of line-paced work were making the job even more difficult. More than half of the men who quit mentioned ageing, illness, and physical fatigue as critical factors in the decision to leave. Here are some of the more typical comments:

> "I wanted to get off the line because I was aggravated by an ulcer condition. I don't mean I wanted to loaf. I like to do a good day's work, but I needed a change. I went to the doctor and he said it was the job that gave me an ulcer, the aggravation, the steady grind. He told me to take a leave of absence. When I came back, I told him that my own doctor said I ought to get a different job. He then said that there were other men on the line who had ulcers, but that there were simply no openings on jobs off the line. The only thing he could offer me was a sweeper's job. I wasn't going to be a sweeper in that place after all those years. I talked to the union. I waited a month for the company or the union to do something. They didn't, so I quit."
>
> "If there was any job where I didn't have to use my back muscles, or where I could have sat down, I would have stayed. But there aren't any jobs like that. The plant doctor examined me and said I had arthritis. My own doctor said that where my back hurt, it couldn't be arthritis. It was from pulling on those grilles and fenders. I was put on a long leave of absence, but I didn't get any better, so I never went back. In other plants of the company which aren't entirely assembly operations, the men can be moved to lighter jobs. They last until they retire—but not on the assembly line."

"I had been thinking about it for five years. I was pretty miserable. You're always under pressure. Finally I got a case of ulcers and couldn't stand it any longer. The longer I worked the harder it got. On the assembly line they don't consider a man for his age and give him an easier job as he gets older. You have to do the same as a younger guy coming into the place."

"The work was too rough for a man to last until his pension."

"I got to be like a mechanical man after a while. At that speed and the movement of your body back and forth, a man becomes a physical wreck after twenty years. Every time I worked a little harder to get 20 seconds for myself, the foreman piled on more work. How long can you last? People just burn themselves out. When things start to speed up and the man can't slow down, it's time to quit."

"I reached a point where I was fed up with production. When you're younger you don't mind. All you're interested in is the pay. When you grow older, you begin to ask 'how long can I stay on the job?' If you're over 40, you can't get a job any place else. In my opinion they treat you like a machine. You either produce or they can't use you. They expect an older person to work as hard as a younger man. They should have a little regard for a man's health. On the job I have now as a man grows older, there are other lighter jobs which they put a man on."

"I had bursitis last fall. When I came back from a leave of absence I asked the foreman to give me a lighter job for a couple of days. He never did get me the job so I just got mad and started looking for a job on the outside."

"I don't think I could last out until I got a pension. The work was too hard. It's all right for a younger man, but not after you pass forty. There were no desirable production jobs. It's all the same no matter where on the line you work. My job was considered a decent job and still I had to work like a jackass to keep up with the line."

One can see in these comments the close relationship between the physiological and the psychological forces on the job. Work was hard, but the pressures inherent in conveyor-paced work made it still more difficult for the men. The question might be raised as to whether these men, in discussing sickness, ageing, and the physical fatigue of work, were merely bringing up physiological complaints peculiar to themselves and not representative of the great majority who still remained on their jobs at Plant Y. While their comments may have been more bitter, they were not substantially different from statements made by the men who stayed.

It is obvious that in any occupation requiring hard physical work, there is always a problem of the older men not being able to carry their work load. In the case of these assembly line workers, however, the problem appears to be more serious because of the rigid demands of the technology itself. A certain amount of work must be performed within an established cycle of time and according to certain work standards regardless of the age or physical condition of the operator. The conveyor is in constant motion and, except for a short breather once or twice a day, every work station must be manned all the time. Notice the comments above which implied that the company deliberately speeded up the line, thus adding more work onto the operator. We have no evidence to substantiate this as a fact. What appears to have happened is that these men, because of their physical condition, were simply not able to keep up with the pace. To the men themselves, this phenomenon had the same physical and psychological effects as an actual speedup.

THE CUTBACK

The immediate decision to quit was further influenced by the circumstances which these men faced at the time the study was made. A cutback in production had taken place which forced some of the men to work temporarily for somewhat shorter work weeks. Such a cutback was quite customary for the industry during a certain period of the year. The monetary effects, however, did not appear to be as important to these men as the fact that they were forced to 'bump' into less desirable jobs. Many of these men had, through seniority, become repair operators or bench operators on off-line jobs. They had slightly more control over the work pace because they were not as directly geared to the line. When production fell off some of the men were, as they put it, 'thrown back on the line'. To the older men or to those having physical difficulties the change was too drastic.

From a long-range point of view the cutback condition was not the primary reason for quitting. All of the men had experienced cutbacks and production fluctuations for many years. This condition coming when it did merely triggered the final decision. All but three had considered leaving for many months and even years before. It was the accumulation of frustrations connected with the job itself plus the attraction to another available job which explains more accurately why they left.

SOCIAL FACTORS

Our enquiry into the reasons why these men quit led to careful examination of the importance of social factors in the job situation. It has long been recognised that a man's relationship to his immediate work group and to his superiors may in large measure determine his overall job satisfaction. Disintegration of satisfying social relationships and the emergence of sharp antagonisms can and often do lead to a decision to quit. Three areas of social relationship are discussed: (A) fellow workers, (B) supervision, and (C) the workers' wives.

A. Relationships with Fellow Workers

One positive, although not the most important, attraction in the former job was the social relationship with fellow workers. Many of the men had worked closely with others for over twelve years. Strong friendships had built up over time. This sense of camaraderie had two aspects to it: (1) the simple satisfaction of working with people you like, and (2) the natural desire to stick together in the face of what appeared as a hostile environment. Following are a few typical quotes, made after the men had left their assembly line jobs, which reveal the strength of the 'social' attractions to the former job:

> "I felt very badly when I left the place. I had worked with the same fellows fourteen years. We started in our early twenties and now we were in our late thirties. You get attached to one another when you have been together that long. Believe it or not I had tears in my eyes when I left those men."

> "The fellows all worked together good. You had to. One man could put us in a hole. We argued sometimes, but it was never serious. We had to be able to talk to let off steam."
>
> "The men got along good together because they are fighting a common enemy—the company."
>
> "I got along good with the fellows. They all hated to see me go. Being a utility man I got around so that I knew the first names of everybody. When you have a regular job the same place on the line, you don't have as much chance to get to know the others. I bet some of them solderers who have been on the line fifteen years can't tell you the name of a fellow ten places down the line."

Nothing in these comments would indicate that 'poor' social relationships between fellow workers was a reason for leaving the job. Nevertheless, the last quotation suggests something which may be significant. A characteristic of most assembly line jobs is that work is not a group function. Each job is performed more or less independently of the next job along the line. There is little functional need for men to interact with one another in clusters, groups or work teams. The amount of informal interaction is limited by the very geography of work stations.

Under such circumstances it was difficult for most of the regular line workers to become identified with a work group and hence with the social satisfactions which go along with such identification. Lack of group identification was not a reason for quitting, but the presence of more positive social satisfactions might have been a deterrent against quitting.

B. Relationship to Supervision

In examining the various forces influencing these men to quit, we were struck by the role played by supervision. Only 43 per cent of the 202 workers who remained held opinions about their supervisors which were at all favourable. In some cases the antagonism between a worker and his immediate boss was a personal one. In most instances, however, the men disliked their superiors because their actions appeared to reinforce the disliked characteristics of the work itself, the conveyor pacing and the sharply defined job cycle and work standards.

Here is the range of attitudes toward supervision reflected in the comments of the workers who finally quit:

> "We got along with our foreman pretty good. If you have a good foreman, you do a day's work. It's a rough job being a foreman. They are all scared of losing their jobs. So sometimes the foremen forgot about the problems a guy has working on the line. They just want the job done."
>
> "My own foreman was o.k. as far as he could be, but some of the foremen and general foremen were lousy. They were always standing around asking why you didn't have anything to do, trying to keep you going on the line all day without giving you a break."
>
> "The foremen were o.k. outside of work but on the job they had to be rough."
>
> "So that you keep up with the line the foreman's got to be after you with a whip all the time. Their bosses were always riding the foreman to keep the men busy. When the foreman sees you with a spare moment, he loads you up."
>
> "I have had good foremen and I would do everything under the sun to do a good job for them. But the one I had last was no good. He had a mind of his own and regardless of what your idea was he wanted you to do it his way. He thought he was much better than the men."

> "The men couldn't stand him for a second. He belonged in Siberia where he could have a whip. The men had to plead with him to get relief. Production was the only thing that mattered—not the men."

These and other comments made by the men who quit indicate that supervision was a source of job dissatisfaction. Nevertheless, only two of the eighteen gave poor supervision as the primary reason for leaving. As indicated before the expressed antagonism toward the boss was often as not a reaction against the frustration of the job itself.

C. *Influence of Workers' Wives*

A discussion of 'social' factors which influenced these workers to quit would not be complete without considering pressures brought to bear from home, particularly from the wives. The comments below, some from the wives themselves, reflect the strength of the home influence:

> "My wife thought I was developing a good case of nerves." His wife added: "That's right, you can't tell from looking at him now, but he is a changed person."
>
> (Wife): "He's a lot more cheerful now. He's better all around. I was so glad when he left."
>
> "My wife didn't know much about my job, but she knew what it was doing to me. There were days when I came home grouchy, all sweated up and hot. She didn't say much, but she was hoping some day I'd get out."
>
> (Wife): "I told him he ought to get out. I didn't like the short week he worked, but also he was always coming home too tired at night and we would have fights about it."
>
> "She didn't like it because I wasn't living like an ordinary man when I worked there. I came home so god damned tired. I had no leisure time."
>
> (Wife): "This thing has been working on his mind for about a year. I felt he should leave because it was so aggravating. The whole household was upset. He is a handy person, but at Plant Y it was just another laborer's job."
>
> (Wife): "I'm happy he quit. He is a different person. He's ten years younger. I'm telling you my home life was miserable. At least he smiles now. He was terrible to live with. The children were happy when he was out."

WHY MEN STAYED: THE DILEMMA OF SECURITY AND INDEPENDENCE

The question may be asked, quite logically, "If these men disliked the intrinsic nature of their work so much why did they remain so long on the job?" This question is discussed in some detail in a separate article drawn from interviews with those 202 men who had remained on the job (Guest, 1954). Their reasons for staying were essentially the same as the ones given by the men who quit. Briefly, the men remained on the jobs for the following reasons:

1. The 'insurance' of seniority and the fear of losing this protection if they quit.
2. The relative good wages and fear of not being able to get a job on the outside paying comparable wages.
3. The fear that 'age' particularly among those approaching forty years old would be against them if they sought employment elsewhere.
4. The fear that the immediate content of a new job might be no better than the one they had.

Those who finally made the break gave the same reasons. Witness the following typical comments:

> "Seniority is the thing that keeps most of them there. It kept me there, too."
>
> "I thought I ought to stay because of my seniority. But when I really got to thinking about it, seniority didn't help, so I got disgusted."
>
> "I stayed because it paid as good as any job I could get—at least it paid good for several years. Now that I have this new job (paying ¼ less) I don't know why the heck I did stay so long."
>
> "Frankly I didn't have the nerve to quit. I was afraid I couldn't get another job. I felt I had a little seniority and I was safe there—and not because I liked the job. If I could see where I was bettering myself with the seniority I had, I would have stayed. But each time they had a cutback, you landed right back where you started."
>
> "The reason I stayed on was the money, and also I couldn't take a chance of going out and looking for another job."
>
> "The money part was pretty good. Also, if I had went somewhere else, I'd lose my seniority. A guy's afraid if he goes somewhere else, he'll be the first laid off if a layoff comes."
>
> "I was afraid to lose my service. That's all that kept me going. I figured maybe next year would be better. That's the only reason I stayed on—the length of service."
>
> "I made a fairly good living. I did better there than I could have done in most places at the time."
>
> "I don't really know why I stayed so long. I must have rocks in my head or something. Seniority, I'd say. In case of a layoff I knew they would have to shut the plant down before they got me. But I knew every day I was there I was wasting my time. There was no future in that place."
>
> "I stayed because I had a lot of bills to pay. I didn't want to leave unless I was assured another job. I applied for another job at another automobile company assembly plant hoping to get a supervisory job. When I checked on it, I found they were going to give me a job on the line, so I didn't quit."

Only one worker amongst the entire group mentioned any positive attractions of his assembly line job as reasons for staying on. His was the only job and one of the only jobs of its kind in all production departments, in which the pace of the work was not closely tied to the speed of the conveyor. His decision to leave was based simply on the fact that he was offered a better paying job on the outside (a job he did not seek out himself) and it was one in which he could make direct use of knowledge and skills learned at the assembly plant.

The emphasis which these men gave to pay and seniority as the primary reasons for remaining on the assembly line job is particularly interesting in light of conditions these men found on their new jobs. It is recalled that the great majority took a wage cut in going into new jobs. None of them had seniority on the new job. Thus, these two factors which were so important to the men as reasons for holding onto the old job seemed to lose their importance on the new job.

What appears to have taken place is that other positive attractions of the new job took on greater importance. The 'security' deriving from seniority alone was changed to a security deriving from a sense of being able to master the job, of being independent. The men no longer were worried about being physically capable of sustaining the job as they grew older.

As for wages, among the majority who took a cut, only one, the bartender, was seriously worried about making ends meet. The rest saw the wage cut as only temporary. They were convinced that in time they would make up the difference. The future looked brighter. By hard work and further training, they believed they could move up the ladder of promotion to better paying jobs. Those who owned their own business saw a steady rise in income coming up in the future.

These attitudes about the future suggest that given a certain amount of independence and control, workers entertain hopes which are in keeping with a deeply rooted American tradition of opportunity and of vertical mobility. Whether they continue to hold to those ideas remains to be seen. For those workers who remained at Plant Y, the validity of this tradition was seriously questioned, at least as far as opportunity in assembly line work was concerned. As the author (Guest, 1954) concluded in his study of work careers and aspirations of 202 workers:

> "The central fact about the work careers of those who remained as hourly workers was lack of change in individual job status. Job rationalisation had all but eliminated gradations in skills and the intrinsic characteristics of the job of any individual remained substantially the same as the characteristics of the job for any other individual.
>
> ". . . Workers do not look for nor do they expect jobs which will give them a higher economic and social status *within* the organisation.
>
> ". . . In the long range picture, the assembly line workers entertain hopes, on the verbal level at least, which are in keeping with the . . . American tradition of opportunity. They want to quit the present job altogether and strike out on their own. Yet to leave means facing the unknown. The present imperatives of security and a reasonably steady income outweigh the attractions of the job world outside."

Here in the study of the eighteen men who did 'strike out on their own' we find these strong feelings about 'opportunity' being expressed, once the move was made. Also we find them minimising the importance of wages and seniority, at least for the present.

SUMMARY AND CONCLUSIONS

The reason men quit their jobs is multi-dimensional. Studies to date have placed much emphasis on the economic and social dimensions of job dissatisfaction. Without minimising either the 'economic' or the 'social' I have tried to show that more attention might be given to those factors about the job which are essentially technological in character. The present study of eighteen workers who quit their assembly line jobs after twelve to fifteen years indicates that the most important source of job dissatisfaction was related to the nature of the work itself, to what we have called the immediate job. Work pace was almost completely controlled by the mechanical pacing of the conveyor. The motions were highly repetitive and each operation was determined by rigid work standards. Even after years of experience these men apparently never became adjusted to this type of highly rationalized and fractionated work. And the reasons for this lack of adjustment are not explained by any inherent personality characteristics peculiar to these eighteen men. It was found that those who remained cited these same job characteristics as primary reasons for wanting to leave if the right 'break' came along.

One clue to the conclusion that the immediate job itself was important came when we examined the kinds of jobs they took when they left. These workers apparently deliberately chose jobs which had completely different characteristics from those of the former assembly line job. Only three of the eighteen went back into factories and not one of the three went on repetitive conveyor-paced work. The new jobs had considerable variety in the sense that identical motions were not repeated in minute-to-minute time cycles. Work pace was in large measure under their immediate control. They worked on the 'whole' of a product or service. The work required more planning, judgment, and skill. In other words, these men got as far away as they could from the work pressures inherent in their former jobs. And most of them took a cut in income to do it.

The dissatisfactions leading to the decision to leave were further compounded by factors of age and physical fatigue. Such factors may be found in any kind of job requiring hard physical work, but they appear to become more exacerbated when there is little opportunity to 'take a breather' or to vary the rhythm of work. Interestingly enough some of the men who quit were working longer hours and using as much physical energy, but they registered little or no complaint about this condition. What appeared more important was the control they could exercise over the minute-to-minute pace of work.

Social factors relating to the former job were explored. A source of dissatisfaction having some but not primary bearing on the decision to quit was the worker's relationship to supervision, his foreman in particular. Attitudes ranged from tolerance to bitter antagonism. The 'boss' appeared to the men as the symbol of pressure. His actions only reinforced the disliked characteristics of the immediate job.

Individual friendships between the men who quit and those they left behind seemed to have been a source of some job satisfaction. Nevertheless, there was little evidence of a healthy positive sense of close camaraderie often found among primary work groups. The reason is largely a technological one. Design of operations virtually precluded any interdependent teamwork.

Social pressures to quit were brought to bear on the workers by their wives. The physical and emotional effects of the job tended to break up what the wives considered to be a 'normal' family life. The new job made it possible for the worker and his wife, together, to engage in more social and recreational activities.

The final decision to quit was for many of these workers initiated by a cutback in production which forced them to 'bump' into even less desirable jobs. This decision to 'take the plunge' was not a hasty one, and it came about only when certain forces holding them where they were gave way under a combination of extreme dissatisfactions on the job plus certain positive attractions of a job outside—not just any job but one which had few, if any, of the characteristics of the job they left.

The reasons for staying were based primarily on a combination of fears. They were afraid of taking a wage cut or of losing seniority. Their age was against them (they thought) in getting or in holding onto another job. They were apprehensive about getting a job which might have similar immediate work characteristics.

The 'hold' which seniority had was the most crucial reason for staying. As he continued to accumulate more seniority, it became increasingly difficult for a man to throw it aside and take a chance on the unknowns of employment elsewhere.

These unknowns largely disappeared when a firm offer for another job was made. When this happened the fears of insecurity disappeared even though certain objective conditions of the new job made them less secure. They had no seniority and for most of the men their income was less. But a new kind of security developed. The men felt 'in control' of the new jobs. They were more independent. They were no longer anonymous cogs in a vast machine. They had greater immediate control over the pace of the work. Future opportunities looked more promising in terms of (a) income and (b) being able to get into a job which was more challenging.

Perhaps more than any other single factor the findings underscore the importance of *what a man does on the actual job* itself. The performance of work from minute to minute in a mass production plant is determined in considerable measure by the nature of the equipment, the flow of materials, and by the engineering rationale behind job design. It is encouraging to note that those who have concerned themselves traditionally with purely physical elements of work—the engineers—are beginning to ask the searching question, "What are we doing to man?"

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Thirty Years of Psychology in an Industrial Firm

By T. M. HIGHAM

Mr Higham, who is Chief Industrial Psychologist, Rowntree & Co. Ltd., York, read this paper at the 1955 Annual Conference of the British Psychological Society.

SOME years ago, in an article on the selection of pupils for different types of secondary schools, Mr Alec Rodger (1949) suggested that there were three characteristics of a good selection procedure. It should be, he said, "technically sound, administratively convenient, and politically defensible". If by 'politically defensible' we mean 'in accordance with a firm's policies', then I believe that these same three criteria apply to almost all the main fields of work in which an industrial psychologist, employed as such, is engaged. Whether it be training or selection, attitude surveys or methods of assessment—all of these must be as technically sound as the psychologist can make them; but he must not forget that the people on whom these techniques will be used are likely, for the most part, to be employed on production work, so that the less time they are absent from it the better; nor for that matter should his methods be too cumbersome or too lengthy; while his terms of reference should be in accordance with the firm's labour or personnel policies.

It is almost exactly thirty years since Rowntrees first employed a psychologist, and in many ways you could regard the history of our Selection and Training Department as a long effort to improve the methods of selection and the techniques of training and assessment that have been used during its lifetime. An account of these was given by Mr W. S. Porteous to the British Association in 1949, and has since been published (Porteous, 1950). An earlier account, by Mr V. Moorrees and Dr C. H. Northcott, was published in 1933, while an account of some newer methods of selection was given at the 1952 Annual General Meeting of the British Psychological Society and published later (Higham, 1952). Consequently I do not want to say very much about the type of tests we give, or the sort of training programmes that we run. I should like, rather, to deal with two main matters—the 'political' aspect of the industrial psychologist's job, and his day to day work; in that way, by taking for granted that any psychologist worth his salt will try to keep his procedures technically sound, I hope to be able to give you a wider picture of how he tries to satisfy the other two criteria I have mentioned.

If I deal with the 'political' aspect first, it is because it is the most important; for a policy decision has to be taken in the first place before a psychologist is appointed; and, as Professor C. A. Mace (1953) has so aptly pointed out, there is more to a decision than just saying 'Yes' or 'No'; for a decision has to be followed through and implemented, and that may require patience and continued effort.

So I should like to start by telling you how our department came into being. As early as 1921, Mr Seebohm Rowntree and some of our other directors had decided that a trained psychologist would be a useful addition to the staff. But in the years following the First World War, labour was very suspicious of anything new-fangled, especially if it could be interpreted as an attempt to make for greater efficiency at labour's expense. But it so happened that a few years previously, in order to put our methods of joint consultation on a more formal basis, a Central Works' Council had been set up. In accordance with the firm's policy, the proposal to appoint a psychologist was first discussed there, where the suggestion was greeted with much scepticism and even hostile criticism. Eventually, the Council agreed to invite Dr C. S. Myers and Professor T. H. Pear to come to the Cocoa Works to give a series of public lectures which would explain how psychology could be of use in an industrial organisation. (There are still a few workers who can remember those lectures; one of them, some time ago, during an interview, paid tribute to 'a chap from Manchester' who had first explained to him what psychology was about.) In the end, the Council approved the principle of appointing a psychologist, and set up a committee to consider what steps should be taken to fill the appointment. This committee, like all of those set up by our Central Works' Council, consisted of members of management and workers. The committee recommended that the Council should agree to the appointment, provided that the person appointed was approved by the committee, and that the type of work which he was to do was defined and similarly approved by it. And that was what happened; looking back on it, from the secure position of the nineteen-fifties, it may all seem rather a fuss about nothing; but at the time it was the only way in which acceptance of the new appointment could have been got.

I believe that it is an important point that from the very start, the workers (on whom, after all, the psychologist's techniques would be used) were brought fully into the picture. As Professor Mace (1951) has pointed out, the good that the psychologist can do "he can do only with the full and free co-operation of an educated and critical laity". 'Black Magic' may be one of our staple products; but we do not believe that, as psychologists, our methods should seem to savour of another kind of black magic. Thus, we always explain our selection procedures, and, for example, on training courses for supervisors, include a session in which we try to make clear exactly what the department does, and how and why it does it.

But despite these preliminaries, there is little doubt that the work of our first psychologist was not easy. I was told the other day that "a wave of horror swept the Works" when it became known that a psychologist was going to be appointed. Nowadays, the appointment of a new psychologist hardly raises a murmur, so well has the department been 'integrated' (as we might say) into the organisation. Opposition was strong in those early days—perhaps because, for a time, motion and time study were included in the department's activities; some of the other departments were unwilling for the psychologist to work in them. But this opposition, like the rest, was overcome in time.

Those of us who are wedded to a too determinist view of the world may well discount the influence of chance in modifying later history; but a fairly dispassionate view of the history of our department shows that luck has played its part. In fact it was by chance that an event happened which was greatly to modify the attitude of workers towards the department. On one occasion, in the early twenties, a number of inefficient workers was given notice. Their case was taken up by the Psychological Department at the request of the men's shop stewards, as it was felt that the men might have been placed in jobs for which they were not fitted. The men were tested and interviewed, and as a result some of them were transferred to other jobs for which they were better suited, and in which they made good. Cases like that rapidly became known and did a great deal to allay the suspicions held by many about the work of the department. Today, too, we are sometimes able to suggest to factory workers that they have the ability and aptitude (as shown by their test results) to carry out clerical work in the offices. In not a few cases we have succeeded in getting such men transferred to jobs which they have found more congenial, and of higher status. We also give vocational guidance both to employees and to their children, if asked. By such means, people come to see that we exist to help our employees to do the work for which they are best fitted, rather than to discover their shortcomings.

It is interesting to notice here, in parenthesis, what Professor O. L. Zangwill once called 'the curiously ambivalent attitude' of lay people towards the psychologist. In the armed forces, for example, commanding officers who expressed themselves forcibly about psychological methods and techniques were often the first to send any problem child that they came across down to the psychologist to see what he could make of him. In much the same way, managers who for perfectly rational reasons disapprove of psychologists will not hesitate to make use of them when the occasion arises.

After two years, it was no longer necessary for the Joint Committee set up by our Central Works' Council to continue in being; but there is little doubt that its restraining influence helped to establish the department and to get its work generally accepted. There is today still some opposition to our work, but it is very slight, and our selection procedures are looked upon by almost all our employees as a normal item in factory and office routine. Last year, when we saw over 2,000 men and women, not more than about 20 to 30 were suspicious about or hostile towards the selection methods. Although this is due in part to our past history as a department, and to the wider use of tests in Britain generally, it is also due to our team of testers, who make the first contact with the applicants and do their best to put them at ease and soften the shock of having to do tests. Here, as so often, the personal factor is of great importance.

Another policy decision which affected and still affects the work is this: the department is advisory; that is, we see applicants and report whether they are suitable for employment in the firm, and if so, in what capacity. But the actual responsibility for employment rests with the managers concerned—either the employment managers in the case of new employees, or the production or other departmental managers in the case of internal vacancies. This may seem like 'the prerogative of the harlot throughout the ages', power without responsibility, but in fact it puts the responsibility where it properly belongs. In selecting a chargehand in one of the production departments, for example, we test and interview all the applicants for the manager concerned. We cannot assess the men for their technical prowess, not being skilled in the techniques of production; our job is rather to set out each man's qualifications, in the way of man-management experience, capacities and inclinations, in such a way that they can be readily compared against the requirements of the job as set out in the job description and analysis. When we have seen all the applicants, we make out a short list; the manager sees all the men on the short list, plus any others he may have in mind, and, after consultation with us, makes the final appointment. By that means the responsibility for the selection is put where it belongs—in the hands of the man's own manager; that is his prerogative and the men's right; they do not want a man put over them who is selected by an 'outsider'. Consultation with the manager enables a fuller view of the man or men concerned to be got than would be possible if either the manager by himself or the psychologist by himself was to make the appointment. The manager, then, makes use of the psychologist's assessment to get a more all-round picture than would be possible otherwise.

In practice, this means that the psychologist's main function is to devise and carry out a selection procedure which will provide the information which is needed to make an accurate assessment and placement. That is where the psychologist's knowledge of appropriate techniques comes in; he must make sure that his methods are sound; exactly the same principle applies to training as well. Past and recent history give us some good examples of this. The earliest tests used in the department were of the pencil and paper type then in vogue; but it was soon found that these were not suitable for people employed in purely manual work, which involved, at times, a high degree of hand and eye co-ordination, as, for example, in decorating or packing chocolates, or in making fancy boxes in which to put them. Tests therefore had to be devised which, while embodying the essential principles of test construction, were presented in a form more closely resembling factory conditions. Some idea of the success of the psycho-physical tests which were developed can be seen from some follow-up work carried out in 1932. Before tests were used at all, the failure rate of girls in one department was 20 per cent. In the nine years between 1922 and 1931, some 1,287 girls were engaged on the basis of test and interview results, and of those, only 122, or 9.5 per cent, were unsatisfactory workers. Half of those were taken on without the agreement of and sometimes against the advice given by the department; thus the failure rate was really cut by three-quarters.

The old test battery, modified from time to time in the light of research, proved satisfactory enough until just after the war; by that time, changes in production methods, and a knowledge of newer techniques developed in the Services, resulted in more sweeping changes. Paper and pencil tests, more acceptable now than previously, for reasons I gave earlier, were introduced again; biographical questionnaires were used in place of the small record cards which had been used before. The older tests were subjected to a rigorous statistical analysis by Mr Roger Garside (now of King's College, Newcastle), with the result that only those of proved reliability and validity were incorporated in a new battery of performance tests. Under the leadership, first of Dr A. Macdonald, who had been with the army selection authorities during the war, and later, when Dr Macdonald became Labour Manager, of Mr W. S. Porteous, who had been on the staff of the Senior Psychologist to the Admiralty, the methods and techniques developed by the armed forces and shown to be of value, were introduced and now have a firm place in our department. In the same way, more emphasis has been put on interviewing methods, while in the training field we make use of the new and proved methods of supervisory and management training. With the current labour shortage, training is perhaps of greater importance than selection, and we are devoting correspondingly more time to its problems and methods. All our methods, both of training and of selection, have to be subjected to follow-up. Mr R. M. Barker, now Training Manager of the Kwinana Refinery in his native Australia, carried out a thorough survey of the results of our induction training of young workers, a pioneer piece of research. It is hoped to publish an account of the validation of three new performance tests at a later date.

Nor can we afford to neglect 'administrative convenience'. The old test battery had taken up to an hour to administer. The newer test battery, more elastic than the old, takes less time, on the whole, depending on the vacancies for which the person is being tested. There are now different tests for different types of applicant—and, a more recent innovation, we also make use of group methods of selection for certain administrative posts.

Two last policy decisions are worth noticing; after the war, the name of the department was changed from 'The Psychological Department', to 'The Selection and Training Department'. This was done because although there was a wider knowledge of psychology among people generally, there was also a greater tendency to confuse it with psychiatry or psychoanalysis; it was felt, quite rightly in my view, that a less emotion-provoking name was more suitable. Secondly, it was decided in 1949 to introduce two trainee psychologists' posts—one for men and one for women—with the idea of giving such people practical experience in industry in a well-established department which would enable them to qualify for more senior posts elsewhere. The experiment has been a great success, as the later history of the one-time assistants has shown.

And now, what is it like to work as a psychologist in an industrial firm? In 1951, at the Annual General Meeting of the British Psychological Society held in Liverpool, Professor Rex Knight (1951) presented the report of the Committee on the Postgraduate Training of Psychologists, of which he was convenor. In that report he suggested that psychologists should be given training "in the necessary social and administrative, as well as the strictly professional, skills". I was rash enough to ask him if he had in mind a course of 'basic lifemanship'; and I was very properly put in my place for introducing a note of frivolity into a serious discussion. After nearly four years in industry, I do not think I would be callow enough to make that suggestion again, for I have learnt to my cost how necessary those social and administrative skills are. Technical prowess is not enough; a psychologist in industry must be generally acceptable to young workers, old hands, supervisors, managers and his contemporaries. We have a blanket term to describe people who are not suitable; we say "They will not go down well in the workrooms"; there is indeed no room for long-haired funnies. Experience too has taught us to beware of the young man or woman who seeks a job on the personnel side because he or she is 'interested in people'. As so often, if you begin by loving humanity you end by putting everyone's backs up. I should also like to stress that work in an industrial organisation bears little resemblance to work in a laboratory; the change from the academic life to the industrial one is apt to come as a shock to the younger psychologist fresh from a university; so it might be as well if I tried to describe to you a fairly typical week in the life of one industrial psychologist.

Monday is fully taken up with interviews; some ten men have to be seen for a vacancy as an enrober operator in one of the production departments. This will have meant a discussion beforehand with the personnel assistant of that department, and a study of the actual job. Interviews, at half-hour intervals, go on throughout the morning and afternoon; one of the men is on night shift and can only be seen after 6 p.m. In between times, there is a report to prepare on a visit paid last week to another firm to see its pre-national-service training scheme; besides all this, there will be other memos to prepare or to study; some technical journals to look through; and just after four p.m. you are suddenly asked to see two boys who have applied for work, as there is no-one else in the department available to see them.

Tuesday is not quite so busy; there are still two interviews left over from the ten men of whom you saw eight yesterday; after that there is a clerical applicant to see; you finish the report you started yesterday and put in an hour's work on an analysis of the test scores and general background of 400 men interviewed for the regular staff. After lunch there are two staff applicants—one for a job in purchasing, one for a post as a production assistant; for both of these, you will have spent some time in discussing the job's requirements with the appropriate managers. After they have been interviewed, reports have to be written on them. At 5·30 you have an appointment in the town with the Youth Employment Committee, of which you are a member.

On Wednesday, you start off by giving a twenty minute talk, followed by a discussion, on 'Workroom Psychology', hoping you can find a better title. (Would 'Why we behave as we do' be more apt, or should you try to be funny and call it 'It's only human nature after all'?) This is to a supervisors' course which the department is running. When you come back, you arrange the details for a T.W.I. job relations course, which you are holding the week after next; there is also a note asking you to carry out a survey on the reluctance of men to apply for a certain job in one of the departments; a little later, you have a long session with people in the Advertising Department about a display to deal with accident prevention. The doctor looks in to discuss a man with you. Can you see him to give him a little advice about jobs? The Education Officer rings up to see if you are free; can he come and discuss the scheme for part time training of boys? After lunch, there is another clerical applicant, and then it is time to go to the meeting of the Central Works' Council, of which you are an administrative member; as usual, you marvel that controversial topics can be discussed with such politeness and good humour. When you return, there are three boy school-leavers to interview; if there is any time left, you go on with your report.

On Thursday, you begin the day with a group selection test for salesmen applicants. You outline the procedure (Higham, 1952) to them and send them off on a tour of the factory; you have no interviews that morning, which gives you a chance to finish off your report, and begin thinking about the study of the reluctant applicants. The morning's post has brought an invitation to lecture on training methods at a technical college, and a request for information about selection methods; you have also been asked to join with the doctor in studying jobs that would be suitable for men with certain medical restrictions. You meet the salesmen applicants at 12.30 and take them over for lunch, discussing television and the prospects of York City. Lunch over, you hand out and introduce the group problem; the applicants study it, discuss it, and then discuss other topics which you give them; when it is over, you confer with the Sales Manager and grade the applicants; then you interview them, confer again, and make a final grading; lastly, you write up your reports on them.

On Friday morning, you see two men for the regular staff; then you prepare a few notes on the week's activities for your weekly meeting with the Labour Manager; as you go through your file, you notice that there are two reports—one on safety instructions, and the other about notice-boards—which should be completed by the end of the week. You have your weekly meeting and discuss several of the topics which you have been dealing with during the week; you explain a point in a report on supervisory training which you sent the Labour Manager the week before; he asks you if you would like to go with him to the T.U.C. in the autumn; you agree. After lunch someone telephones to ask if you can arrange a group test for the week after next; you find you can't, so have to arrange it for three weeks' time; you make a note to prepare a new problem. You also arrange times for test and interview of 20 candidates for a leading hand's post . A book is brought in to you, with a note to read Chapter V and see if there is anything in it of value to the firm; there is also a man to see who feels that he was hard done by in a recent selection. And just to finish the week, you return once again to your draft of the annual report, which you have been trying to complete for some weeks; you last looked at it two weeks ago.

From this brief and hurried picture (which is based entirely on fact and our engagement pads) you can, I think, see that to work as a psychologist in industry calls for a very non-perseverative nature; it is rarely possible to spend a whole day, let alone a full week, on some problem. For example, one important piece of research—on the validation of three performance tests first introduced some seven years ago—had to be spread out over several months and fitted in when time and other engagements allowed. All our work involves making contact with many different types of people, while interviewing often involves switching from one applicant to another of totally different background, or for a different vacancy. There is certainly plenty of variety in the job, and time does not hang on our hands.

But I hope that you will not think, from these scattered fragments of past history, that we feel we have no more to learn, no more difficulties to overcome. There is still plenty of room for advance and enterprise, for each new research often opens up exciting new possibilities. None the less, it is true to say that we can only do our work effectively if people are convinced that what we do is reasonable and fair. In our 33 years of existence, I think we can say quite truthfully that people are convinced of those points; and I cannot do better than end with a passage from the late Seebohm Rowntree's book *The Human Factor in Business,* in which he sums up his views on the work of the department. It was written in 1938, but I believe that in substance it remains equally true today. He said: "Today, vocational selection is looked on throughout the works as a normal item in the factory routine. It is generally recognised that by helping to fit individuals into the jobs for which they are permanently suited, vocational selection has given them a larger degree of happiness and satisfaction in their work than would formerly have been possible."

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Incentives

By SYLVIA SHIMMIN

Miss Shimmin, who is a member of the Medical Research Council's Industrial Psychology Research Group at University College, London, read this paper at the 1955 Annual Conference of the British Psychological Society.

"AN incentive is an objective goal which is capable of satisfying what we are aware of subjectively as a need, drive or desire." (Brown, 1954.) Like all terms which gain in popular usage, the word 'incentive' has been given different meanings by different writers. Most would agree with the definition quoted above in stressing that an incentive is something objective for which the individual strives, and in many instances the term is made synonymous with 'goal'. More specifically, however, incentives refer to objects in the external situation whose function is to increase or maintain some already initiated activity either in duration or intensity. Therefore they may be goals to be attained in themselves, or additional rewards which will enhance the satisfaction following attainment of the original goal. Deterrents are objects which have the reverse effect, which impede rather than facilitate behaviour. There is some confusion of terminology in descriptions of the internal state of the organism associated with the external incentive. Bartlett (1950), for example, speaks of internal incentives, but others fail to maintain this distinction, although it is generally agreed that there is both a subjective and objective aspect of motivation. The subjective factors involved are usually inferred from the external objects which are found to satisfy the individual's needs, rather than from more direct information such as the conscious desires of human subjects.

Within a laboratory situation it is possible not only to identify, but to measure, some of the incentives which will lead to an improved performance in a given task, and to compare their relative efficacy for different individuals. For example, Grindley and his collaborators (1938, 1949) demonstrated the beneficial effect on subjects' performance of supplying them with the knowledge of results of previous trials. In the field of industry, however, it is impossible to isolate and manipulate variables in this way because of the many uncontrollable factors which are always present. Neverthless, this is a sphere where incentives are continually discussed and, as such, it forms the background to this paper.

The classic model when discussing industrial motivation used to be that of the donkey with the carrot in front of him and stick behind. Although this view is now regarded as obsolete, its familiar form may be detected behind many current discussions of incentives. It is assumed that without some external goad or reward no-one (and a factory worker in particular) will work at anything approaching his full capacity, although it may be questioned how far this is a rational assumption; and it may, indeed, be largely prejudice. The view is not peculiar to management but is shared by a number of those on the shop-floor. About 25 per cent of workers interviewed in the last year were of this opinion, although the man who said "no working-man will ever work for work's sake" was putting it more forcefully than the rest. Following this argument the criterion of an incentive for both workers and management appears to be something which makes a man do more work than he would usually feel inclined to do; not a very satisfactory criterion perhaps, but one which suggests that further research into the bases of the implied underlying subjective standards would be profitable.

Similar reasoning is probably responsible for the many and varied 'incentives' judged to be operative in industry today. A typical list groups together 'fair wages, 'sickness pay', 'knowledge of results of performance', and 'pleasant relations with associates', among others. Although each one of these items is undoubtedly important in the total situation, to place them all under the general heading of 'incentives' is misleading. As Baldamus (1951) has pointed out, many discussions of incentives confuse objective and subjective aspects of motivation thereby creating sham problems. For example, from time to time one comes across fruitless arguments as to whether or not incentive payments are more effective motivating factors than, say, 'team-spirit'. The position is clarified if we restrict the term incentive to those things which can be manipulated and controlled, *e.g.*, knowledge of results, financial rewards, and regard intangibles such as 'sense of justice' and 'fair play' as the residuals or by-products of other actions. These residuals cannot be manipulated as independent variables and, although they may be consciously perceived as the desired ends, there is no objective standard by which to judge their achievement comparable with that for material rewards.

It is probably because they can be handled so much more easily that financial rewards retain their prominence as industrial incentives. Various studies in recent years have shown that what may be described broadly as the ego needs and social needs of the individual have to be met in the working situation, *e.g.* the need for approval and recognition from his fellow-workers, desire for prestige and status within the group, are increasingly recognised as important motivating factors. Nevertheless, the usual procedure adopted when it is desired to 'increase worker motivation' is to apply some form of bonus payment. It is useful, therefore, to discover how far financial rewards of this kind form satisfactory incentives, and to what extent they are vitiated or enhanced by the more intangible factors in the situation.

The basic principle of any form of wage-incentive is that of 'payment-by-results': the individual worker, or a working-group, is rewarded commensurately with performance. Theoretically the main requirement for its successful application is that an accurate measure can be made of the work done. In practice, however, features of the working situation, such as the nature of the task and the type of payment system adopted, have to be taken into account. For example, where the method of bonus payment is on an individual basis the system may be valued because each person is responsible for his or her earnings, and it prevents slacking, but for precisely the same reasons it may be disliked as putting too great a strain on older workers.

Thus one man gave his opinion of bonus schemes as follows: "I can't come down for or against really. I think a half-way position is needed. If you've no bonus you'll have slackers round you, but the high production needed to earn the money comes hard on the older men."

Other conflicts found in association with an individual method of payment derive from interactions between social and organisational factors. It frequently happens that, although all the work done in a particular department is of the same kind, some jobs are more difficult than others and have to be given to the more experienced operators. Unfortunately, the characteristics of the jobs which render them difficult, such as unpredictable variations in the materials used, are not easily allowed for when setting a standard for the operation. As a result a 'hard' job is often one which not only requires more skill, but is also one which will earn less bonus than a more straightforward task. Consequently a sense of grievance and unfairness is felt when new operatives earn more money on the 'easy' jobs than the experienced workers on difficult operations. The situation becomes more complicated when the bonus payment represents a large proportion of the weekly wage. In the words of a foreman: "The new ones have got to live just as much as the old hands, haven't they? And the money's no good without the bonus, so what can I do but work it this way?" It will be appreciated that attempts to make a fair allocation of the 'good' and 'bad' bonus jobs can add considerably to the work of a supervisor, and social relations throughout his department will be influenced by how far he is judged to do this successfully.

Where financial incentives are on a group basis, the converse of some of the above conflicts is found. Dependence on others is disliked on the grounds that there is no individual control over the amount earned, and because the lazy and the conscientious members of the group are rewarded equally. On the other hand, the absence of friction resulting from large individual differences in earnings and the co-operative effort in working for a collective bonus are often appreciated. It should be noted here that 'group' when used in this connection has no precise meaning either in terms of numbers or social organisation; it may refer to perhaps half a dozen people who work together as a team, to a whole department of over a hundred, and, in a recent example, to a numerically small group on shift-work, two members of which never see each other at all. Therefore many problems arise which are specific to the organisation of the groups and the type of incentive payment system applied. A penetrating analysis of psychological conflicts caused by group bonus methods of payment has been given by Norah Davis (1953). She found that as well as creating social conflicts within or between payment groups, an individual might be torn between a desire to earn bonus and a desire to work at a more leisurely rate in the interests of health; that personal standards of workmanship and quality often conflicted with the desire for speed and quantity; and that the tendency to capitalise on self-interest at the expense of others was often at variance with a personal code of ethics.

Data obtained in our current enquiry into the effects of incentive payment systems support these findings. They reveal the need for more detailed studies of working groups with regard to the value associated with work and leisure, and the effect of group standards of performance on individual behaviour. Why is it, for example, that groups of girls on a finishing and packing operation try to achieve an inordinately high target—one-third above that expected by the firm—on at least one day each week for the privilege of leaving ten minutes early on Friday afternoons? In the factory where this occurs no-one can now recall when or how the practice originated but it is an established tradition of that department. It is probable that the individual and group needs for status, prestige, and social collaboration which are satisfied in reaching this goal are more important than the unofficial, institutionalised reward of ten minutes. However, as the latter was chosen by the girls themselves it suggests that more consideration should be given to the increase of spare time as a possible incentive. This view is reinforced by the situation in another firm, where a group of men prefer to ignore all rest periods until they have completed their daily allocation of work, after which they may have as much as an hour and a half at the factory in which to do what they like.

A great deal is heard about a 'normal' day's work, 'average' efficiency, 'a fair day's wage for a fair day's work' and so on in discussions about work and wages. Thus, many of those who are responsible for incentive payment schemes claim that their purpose is to maintain a reasonable performance from their workpeople, *not* to induce extra effort. The workers, on the other hand, may retort that the wage incentive is merely a device for getting more work from them than they feel is desirable. It is obvious that evaluations of this kind defy objective measurement and operational definition; it is equally clear that those who use such phrases are often arguing at cross-purposes without knowing it. However, the interesting point for the psychologist is that the people concerned have *subjective* standards as to what constitutes a day's work, about which comparatively little is known. These standards are undoubtedly complex, and are probably influenced by factors such as habitual working tempo, amount of fatigue felt at the end of the day, the conscious effort involved, and more subtle social considerations, for example if the job is one which was done by women during the war. It has occurred that men have left a job because it does not enable them, in their view, 'to do a day's work', but what is meant by this assessment or how it comes about is not known.

The conclusion reached is that more attention must be paid to what has been described as the 'subjective' aspect of motivation. In popular terms incentives are thought of as devices for mobilising the 'will to work', and though Maier (1946) and others reject this as a useless concept, it is not easily discarded in the applied field; indeed, Viteles (1953) makes extensive use of it. When a worker speaks of something giving him an 'incentive' he is usually referring to the effect on his attitude towards the work itself, and not to a specific reward sought for its own sake. One feels he might equally well have said 'it encourages the will to work', and have meant the same thing. The latter is the layman's term, and, although it is not popular in present-day psychology, if we wish to increase our knowledge of motivating factors in the working situation we shall be well-advised to examine it more closely.

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An Age-Analysis of some Agricultural Accidents

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SUMMARY

REPORTS on investigations into age variation in accident rates among industrial workers have usually presented data showing overall frequencies of accidents for a department, a factory, or an even wider area. Apart from difficulties which arise through inadequate control of exposure to risk, such data are of little assistance as a guide to practical measures of accident prevention, since no account is taken of age differences in kinds of liability. In this study an analysis was made of nearly 2,000 accidents to workers in agriculture, where there is a wide range of tasks and situations, and a corresponding variety of kinds of accident, to see whether age differences could be established. The results indicated that the prevalent kinds of accidents varied with age, significant differences being found for accident causes, the nature of injury, and the part of the body injured.

THE PROBLEM

During the last thirty years there have been a number of studies of occupational accident rates in relation to age, most of which have led to the conclusion that as workers become older they become less liable to accidents. Careful examination of the relevant articles shows that as a rule insufficient allowance has been made for the operation of important influences such as selection of workers over the years, experience outside the factory under study, and differences in age between the population on one kind of work and that on another (King and Speakman, 1953). In addition, the overall rates of accidents in departments or in whole factories have been considered without analysis of kinds of accident, location and nature of injury, and the like.

The present state of affairs is no doubt largely due to the inadequacy of the information available, for few sets of data measure up to the requirements listed by Whitfield (1950) as necessary for a thorough study of accident causation. However, it has been contended elsewhere (King and Speakman, 1953) that the exact shape of an accident curve for age will vary according to the nature and stresses of the job and the working conditions. If this view is correct, the analysis of a sample of accidents occurring in a number of different situations into separate kinds of accident might be expected to reveal age differences. As a further consequence, it might be possible to indicate situations in which the environmental stresses fall at points where age changes increase older people's risk of injury, or places where their capacities are such as to enable them to avoid the hazards of a job to a greater degree than a young person. Any such results, even if only tentative, would be of value at a time when much emphasis is being placed upon the extension of employment opportunities for older workers.

Where the great majority of accidents in a department are of a similar nature, as was found by Newbold (1926) to be the case in the making of tin boxes, to give only one example, it may well be that a result will be obtained which appears to show a definite age trend in accident frequency. In such a case, in the absence of detailed analysis, other trends are likely to be obscured because they are only to be seen in small groups of cases which are outweighted by the predominant group. In a situation where there is the possibility of numerous kinds of accidents, there is a greater chance of observing such trends, provided that the sample is sufficiently large. Agriculture happens to be a place for study where these hopes are fulfilled, since, during the course of the farm year, many different techniques are used, and the organisation and conditions of the work provide opportunity for a variety of accidents. While, for reasons which will appear, the records available for agricultural accidents were not ideally suitable, the analysis of them provides evidence for the existence of several different age trends in accident rates, rather than a single overall trend.

SOURCE OF DATA

One of the two trade unions having agricultural members in branches throughout England and Wales supplied details of 1,991 cases of injury incurred by farm workers during the course of their employment. Apart from 127 cases in which injury, usually in the form of strain or synovitis, was not the result of an 'accident' in that it did not have a sudden onset at any particular moment, the sample is made up of injuries through accidents in the strict sense of the term. The cases supplied were those dealt with during the period January 1st to July 31st, 1949. (Since 1949 the number of cases dealt with has increased considerably.) On the basis of total membership figures, at the time about 130,000, it would seem that there were about 2.6 reported accidents per 100 members per year. The necessity for calculating such figures from 7 months' experience arises because the original plan for analysing a whole year's records had to be curtailed, the task of extraction proving to be too heavy for the union's staff.

When a member had received medical attention for an injury received in the course of his employment, he filled in a form giving personal details and information concerning the accident, and could then obtain accident benefit if necessary; in addition, questions of insurance and compensation, if such arose, could be handled by the union. There was no qualifying period of disability before an accident was recorded, and so far as this study was concerned no account was taken of the possibility that a man might have sent in forms for more than one accident during the period. From the records were taken for analysis the particulars relating to the age of workers, the location and nature of injury, and a brief description of the circumstances under which the accident occurred. The following are examples of the extracts received:

AGE	INJURY	CAUSE
52	Bruised shoulder	Fell from load of straw
28	Fractured wrist	Starting tractor
44	Injury to knee	Kicked by cow

There was no information upon the length of service or of experience, or the man's occupational designation (the last would in the case of farm workers have been of only limited value, because of the variety of tasks and their overlap from one designation to another), and in a number of cases the reports were not so precise as was desirable. The length of disablement was not a subject of study, but a number of previous workers have shown that accident severity in terms of time lost rises steadily with age. It will be seen from this description that the data fell far below the ideal, and analysis and conclusions must be correspondingly limited. We cannot calculate the frequency of accidents by age groups in the population at risk, since the age-structure of the union membership is not known, and it is therefore necessary to work on a basis of internal comparisons within the overall data. However, comparison can be made (Table 1) between the age distribution of the injured persons and that of the insured population in agriculture at July, 1948. The distributions follow roughly the same pattern but the proportion of accidents to workers is relatively small in the under-21 age group, which may be attributable in some measure to low union membership at these ages.

CAUSES OF THE ACCIDENTS

While accident prevention in agriculture is still at a stage where the detection of the points at which mechanical and other hazards exist, and the consideration of what measures should be taken for their removal, are of major importance, we are mainly concerned with the human aspect of the problem and the analysis has been framed with this in mind. There are some cases which human capacity could not have foreseen or escaped, but such 'no human factor' cases form only 1 to 2 per cent of the total. Table 2 gives the percentages by age groups of cases falling in various categories of cause, the table being divided into sections according to the nature of the age

TABLE I: AGE DISTRIBUTION OF ALL ACCIDENT CASES AND OF ALL INSURED WORKERS IN AGRICULTURE,* JULY 1948

AGE GROUP	NUMBER OF ACCIDENTS	PER CENT OF TOTAL	PER CENT OF ALL INSURED WORKERS
15—20	157	7.9	13.7
21—30	415	20.8	23.2
31—40	469	23.5	22.1
41—50	449	22.6	19.4
51—60	318	16.0	12.6
61—80	183	9.2	9.0
Total ..	1,991	100	100

*The figures for insured workers were calculated from Ministry of Labour estimates, and exclude employers, workers on their own account, farmers' sons and certain other near relatives, who were at that time not insurable under the Unemployment Insurance Scheme. Men aged 65 and over were also excluded, but an estimate derived from Ministry of Agriculture returns (June 4th, 1948) has been included for this group.

TABLE 2: CAUSES OF ACCIDENT

CAUSE	NUMBER OF CASES	PERCENTAGES OF EACH KIND OF ACCIDENT BY AGE GROUPS						SIGNIFICANCE OF AGE TREND (P)[1]
		15–20	21–30	31–40	41–50	51–60	61–80	
INCREASES WITH AGE								
Falls from heights or machines ..	252	7.6	8.4	10.5	14.5	19.2	16.4	<.001
Falls through slipping or tripping, on ground	201	3.8	6.3	9.4	13.4	11.0	16.4	<.001
Hit by falling or moving object	339	14.0	15.7	13.9	17.6	21.4	21.3	<.004
DECREASES WITH AGE								
Caught in machine ..	202	13.4	11.8	11.5	9.1	7.9	6.6	<.002
Injury inflicted by own tool	184	13.4	11.8	10.7	5.8	6.6	9.3	<.001
Continued activity ..	127	4.5	9.2	7.9	6.2	2.8	4.4	<.001
Starting an engine ..	98	12.7	7.9	4.1	4.5	1.3	1.1	<.001
NO SIGNIFICANT CHANGE WITH AGE								
Moving heavy objects	103	3.2	4.8	6.8	4.2	7.5	1.6	>.1
Knocked against, or trod on, object ..	90	6.4	3.6	5.3	4.9	3.4	3.8	>.1
Action of animals ..	79	1.3	3.9	4.3	4.2	5.3	2.7	>.09
Trapped, other than in machine ..	48	1.3	3.1	1.9	2.7	1.6	3.8	>.1
Miscellaneous ..	127	9.5	6.0	7.7	4.7	6.0	6.0	—
Cause not specified ..	141	8.9	7.5	6.0	8.2	6.0	6.6	>.1
Total	1,991	100	100	100	100	100	100	

1. The test for significance used was Whitfield's (1947) modification of the *Tau* method of rank correlation (Kendall, 1948), whereby the total number of cases was divided into those of a given type and those not of that type, followed by ranking into 5-year age groups. In this table, however, for conciseness the proportions are given by 10-year age groups. All ages over 60 were taken as one group.
2. Some cases in which animals were involved were classified under ' hit by falling or moving object.' This group constitutes the remainder.

trend. There was a sizeable number of cases in which the description was too vague for them to be classified, but this group does not show any age bias. A number of groups of causes, each with less than 40 cases, and thus too small for satisfactory treatment, have been placed together in the table.

The groups of causes which are seen to be particularly prevalent among the older persons correspond to those listed in the report of the National Advisory Committee on the Employment of Older Persons (1953) as kinds of accident to which older industrial workers are more liable than younger ones. Also mentioned in that report were injuries from works transport, but very few cases of this kind occurred among the agricultural accidents. Sheldon (1947) and Droller (1955) have independently found a high incidence of tendencies to fall among elderly persons whom they visited, but it is interesting to note that the increase here observed in such accidents begins at ages far below the lower limits of their samples. Droller considers that involutionary changes in the central nervous system are ultimately responsible for the trend, leading to slowness in recognising the hazard, in deciding what action to take, or in organising and executing the muscular response. Such slowness is in line with what is known of sensori-motor performance by older people in general, and could also account for the increase with age in accidents due to being hit by a falling or moving object. If this is true, sensori-motor slowness, whatever its ultimate cause might be, could account for all the categories of accident which show a significant rise with age.

The kinds of accident so far considered occurred in a wide variety of situations and during the execution of many kinds of task, so that it was reasonable to regard the trends as being in fact due to increased liability with age, rather than to special environmental factors. So far as the decreases with age are concerned, it is less easy to eliminate the influence of variation in the age distributions of men on different kinds of work. Tractor drivers have a relatively high exposure to risk of 'starting an engine' and 'caught in machine' accidents, and workers of this kind are mainly younger men (King, 1953). This may not be the only factor, however, since accidents of both these kinds can be greatly reduced in frequency if sufficient care is taken and use is made of precautionary measures. Increased experience in general, and a refinement of skill in the use of the tools, in addition to greater care, are the probable explanations of the decrease with age in accidents due to mishaps with hand tools. The same appears to be true for 'continued activity' injuries, incurred through the repetition of a task to an extent to which the part of the body concerned was not accustomed, or through failure to throw the load on the right muscle groups. In these last two categories it is improbable that the observed trends can be accounted for in terms of differences in age distribution of men on various types of work, because of the widespread occurrence of the tasks involved.

The absence of age variation in the incidence of injuries arising during the lifting or moving of heavy objects is interesting, since strength as measured by a variety of tests has been shown to decline with age from the late 20s onward (Cathcart *et al,* 1935; Fisher and Birren, 1947). If older people were avoiding the heaviest jobs to a substantial extent, in a way similar to that observed by Richardson (1953) among workers in heavy industry, one might expect a decrease with age in injuries of this kind. On the other hand, if older men carry on doing the heavy jobs while finding them difficult it would be expected that there would be an increase with age in these accidents. It is possible that both factors are in operation and are more or less balancing.

PART OF BODY INJURED

Analysis of the sample according to the location of injury again reveals strong age trends, particularly a decrease with age in the proportion of arm injuries, which form the largest group. The figures are given in Table 3. Injuries to the trunk skeleton and musculature increase markedly with age, and leg injuries do so to a lesser extent. Falls predominated among the causes of trunk injuries, accounting for 182 cases, and were particularly numerous at the upper end of the age scale. For arm injuries, several groups of cause were of major importance; namely, caught in machine (161 cases), injured by own tool (135), hit by object (90), and starting an engine (74). The first two categories predominated among cases of injury to fingers and thumbs, accounting for 209 of the 370 cases; the last accounted for 44 of the 133 wrist injuries, where falls and continued activity were also important. Injuries to the hand and to the arm above the wrist were less dependent on any single group of causes. Falls accounted for 31 per cent, and hits by objects for 29 per cent, of the leg injuries, and their effect was seen when the region was subdivided, although with some differences in relative importance. Thus, falls caused twice as many knee and ankle injuries as did hits by objects, while the reverse was true for foot cases, and nearly so for the other parts of the leg.

TABLE 3: LOCATION OF INJURY

LOCATION	NUMBER OF CASES	PERCENTAGES OF EACH REGION OF INJURY BY AGE GROUPS						SIGNIFICANCE OF AGE TREND (P)[1]
		15–20	21–30	31–40	41–50	51–60	61–80	
INCREASE WITH AGE								
Trunk, skeleton and muscles	406	10.0	12.2	20.9	22.4	27.5	22.9	<.001
Leg	560	24.4	23.9	28.1	28.6	26.3	34.0	<.04
DECREASE WITH AGE								
Arm	814	53.8	51.2	38.8	33.8	31.7	31.9	<.001
NO SIGNIFICANT CHANGE WITH AGE								
Head and neck	92	5.6	4.7	3.7	4.3	5.5	3.7	>.1
Eye	66	3.1	2.8	2.7	3.7	3.9	3.2	>.1
Trunk, internal organs	65	2.5	2.6	3.3	3.9	3.0	3.2	>.1
Region not stated	48	0.6	2.6	2.5	3.3	2.1	1.1	>.1
Total[2]	2,051	100	100	100	100	100	100	

1. The method of statistical analysis was the same as for accident causes.
2. The total here exceeds 1,991 because a few accidents caused injury to more than one region.

It is clear that there is often a strong relationship between the type of accident and the part of the body likely to suffer. In general, the kind of accidents which young people incurred tended to be connected with the job in hand and to result in the injury of the member concerned. The kinds of accident to which the older people were particularly liable, falls and hits by objects, frequently resulted in injury to parts of the body not directly involved in the activity preceding the accident.

TYPE OF INJURY

While up to this point there has been little previous work to compare with the present results, there have been some studies of age and type of injury through accident which provide a useful check on the findings. The figures for various kinds of injuries are given in Table 4, where it will be seen that a difficulty encountered during the analysis was the existence of a considerable number (641) of cases where only the words 'injury to . . .' appeared in the accident report. It was found that as age increased, so did the likelihood of such a vague report.

TABLE 4: NATURE OF INJURY

INJURY	NUMBER OF CASES	PROPORTIONS OF EACH KIND OF INJURY BY AGE GROUPS						SIGNIFICANCE OF AGE TREND (P)[1]
		15–20	21–30	31–40	41–50	51–60	61–80	
INCREASE WITH AGE								
'Injuries'	641	25.0	28.0	29.6	36.0	34.9	34.9	<.001
Bruises and contusions	112	4.4	3.3	5.9	6.4	5.8	8.2	<.02
DECREASE WITH AGE								
Cuts and lacerations	334	22.5	20.9	16.0	11.8	15.3	16.4	<.001
Crushings	99	5.6	6.6	4.6	4.8	4.3	2.2	<.02
NO SIGNIFICANT CHANGE WITH AGE								
Fractures	309	19.4	15.6	12.4	14.3	18.3	15.3	>.1
Sprains and strains	223	7.5	10.0	13.0	12.9	8.9	10.4	>.1
Punctures	133	7.5	5.9	8.6	5.5	5.8	6.0	>.1
Hernia	53	1.9	2.4	2.1	3.3	2.7	3.3	>.1
Miscellaneous	120	6.2	7.3	7.8	5.0	4.0	3.3	—
Total[2]	2,024	100	100	100	100	100	100	

1. The method of statistical analysis was the same as for accident causes.
2. The total exceeds the number of accidents because in a few instances more than one injury was incurred.

The decrease with age in the frequency of cuts and lacerations, and also the similar tendency observed for the small number of burns and scalds, where 17 of the 20 cases occurred among men under 40 years of age, is in agreement with that found by Barkin (1933) among a group of males compensated for temporary disability. Stevens' figures (1929) on age and length of disability among 5,000 compensation cases, when used to calculate proportions by age groups, give the same result. Barkin suggested that the decrease in cuts and lacerations was due to the fact that "the older individual masters his tools and becomes more proficient and careful at the job". Among the agricultural cases, 80 per cent of the injuries caused by faulty manipulation of a tool were cuts and lacerations, forming 45 per cent of all injuries of that kind.

The other investigators' results also show a decline with age in the proportion of punctures, but these would have been in the main inflicted by tools, whereas among agricultural accidents many were caused by thorns, and arose very differently. Barkin's figures for fractures show a general upward trend with age, but in our data the highest proportion occurred in the 15-20 age group, followed by a fall to the minimum in the 31-35 group, with some tendency to rise thereafter. No groups of causes predominated, so that it is difficult to suggest reasons for the differences in the results. However, it is interesting to note that Padley (1947), in a study of injuries to Scottish insured workers, did not find an increase with age in fractures among men, although he did so among women.

Bruises formed 36 per cent of the injuries in Barkin's sample, against 5 per cent here, but in both instances an increase with age was observed. The existence of the large group of 'injury cases' may partly account for the large difference in proportions, since it seems reasonable to assume that many of these were in fact bruises or contusions, where the disability is of a less definite kind than it is for, say, a fracture or a cut. For both the groups of 'injuries' and of bruises and contusions, falls were the largest group of causes, followed by hits by objects, the two kinds together accounting for more than half the cases.

The analysis of type of injury, although insufficient to provide more than pointers to the reasons for the observed trends, again shows that accident trends with age may differ markedly according to the nature of the accident considered.

CONCLUSION

Previous studies of accidents in industry have given results which are either conflicting or are liable to misinterpretation because no proper account has been taken of different exposures to risk of older and younger people. The collection of data from which the influences of variable exposure to risk can be completely or even largely removed presents very considerable difficulty and could not be attempted here. On the other hand, practical measures to prevent accidents should take into account the actual accidents sustained, their causes, their nature, and the parts of the body injured. Insofar as these differ with age different measures of accident prevention are needed for younger and older persons. However, the overall accident statistics which are usually presented give no indication as to whether the causes and nature of the accidents differ with age. While suffering from a number of limitations, the present data indicate that they do, whichever criterion one adopts, cause, nature of injury, or part of the body injured.

ACKNOWLEDGMENTS

The grateful thanks of the author are due to the trade union staff who collected the accident records, to Mr A. T. Welford, Director of the Nuffield Research Unit, for much helpful criticism and advice, and to Mr D. Speakman for help with the literature on age and accident rates.

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Book Reviews

The Sociology of Work. By THEODORE CAPLOW. Minneapolis: University of Minnesota Press, 1954. Pp. viii+330. Price $5.00.

"This book", the author tells us, "was originally intended as a description of occupational institutions, but it has gradually turned into an essay on the division of labour." It is, indeed, a very interesting essay; and amply demonstrates that the ideas on this subject first conceived by Adam Smith remain rich in undeveloped possibilities.

Professor Caplow poses a number of very fundamental questions: What are the principles that actually determine, and what are the principles which might reasonably determine, the division of labour according to sex, age and skill? What determines occupational status—the status of an individual within an occupation and the status of his occupation in relation to other occupations? What are the forces that determine occupational mobility?

The questions raised in this book are more exciting than are the answers given. Some readers may be tempted to appraise it as a brilliant demonstration of the art of making bricks with very little straw. This, however, would be to miss the most significant features in this study. The author may fairly claim to have shown that many problems long discussed by economists and psychologists can be restated as questions of general sociology, and as so restated quite new lines of research have been opened up. A new approach is suggested to the theory of the labour market, to the study of Trade Unions as 'associations' in the sociological sense of that term. Sociology itself is advanced by the special study of occupational mobility of occupational 'institutions' and occupational ideologies. So, too, many questions of occupational psychology—vocational choice and the causes and effects of changing conditions of work—take on quite a new look when approached from a sociological point of view.

The division of labour in industrial societies, Caplow suggests, come about as part of a three-fold problem of *aggregation, differentiation* and *rationalization*. The two first of these terms are sufficiently self-explanatory. Aggregation, the increase in the sheer mass of social groups, is illustrated by the fact that between 1840 and 1940 the proportions of the U.S. population living in cities of 100,000 or more increased from 3 per cent to 28.9 per cent. More striking perhaps is the fact that there are now single industrial organisations employing 75,000 individuals. Differentiation is exemplified in the fact that whereas occupations classified by the 1841 Census in Great Britain listed 431 occupations, the Occupational Dictionary compiled by the U.S. Census Bureau a century later coded 25,000 occupational titles.

'Rationalization' is not the best of words for describing the substitution of formal and standardized procedures, devices and relations in place of the informal, personal and variable procedures, devices and relations which arose spontaneously in unplanned social situations. In the growth of an industrial organisation, as when for example a small odd job man working on his own expands his activities, the first step in 'rationalization' is perhaps in the keeping of accounts. To begin with all accounts may be kept in his head, but he has soon to introduce the formal standard procedure of accountancy and book-keeping. This kind of change progressively extended to every phase of management and every branch of industrial and commercial activity—planning, progressing, recruitment, training, promotion, provision for retirement and so on. This process has been carried much further in the United States than elsewhere and 'the essential quality of rationalization' is best brought out by extreme cases. Caplow cites a training system for salesmen in which instruction and drill is provided in gesture and action and in which even the content of the casual conversations with customers is specified, including 'suitable comments on the weather and prefabricated reminiscences'. We can see what he means but may well doubt whether 'rationalization' is quite the *mot juste* for describing the phenomenon.

The extreme cases draw attention to what is surely an important general principle, *viz.*, that there are definite limits to the extent to which a procedure can be 'rationalized', and that the most effective procedure will combine informal and formal elements. A measure of informality is required to act as a lubricant, so to speak, for the formal machinery. Standardization beyond a certain point may defeat its own purpose. A standardized 'casual conversation' is almost a contradiction in terms, as 'rationalization beyond reason', might perhaps be adopted as a definition of 'red-tape'.

The distinction between formal and informal procedure has many important applications. The generalized concept contributes to an understanding of the related distinction between 'formal' and informal organizations. Caplow, having no doubt in mind the famous Hawthorne studies as a recent 'discovery' (quotes his) of industrial sociologists, goes on to say that informal organization is an almost furtive adjunct to official organization and suggests that strong informal organization will generally have as its chief function the subverting of the rational ends for which the formal organization is designed.

This is surely a mistake arising from a failure to distinguish the anomalous from the normal. It is anomalous and a sign of defective management when informal organization runs counter to the formal organization of a firm. The normal function of informal organizations is, at least, as indeed Caplow says in the following sentence, 'to soften the impact of rationalization'. Its function, however, is much more important than that. Informal organization is the fluid matrix, the mother liquor, out of which formal organization has crystallized, and by which and in which it is sustained.

In other words no organization can be purely formal. Even in the most highly formalized organization good co-operation depends upon a great deal of informal give and take. Morale in a fighting unit clearly cannot be maintained merely by the punctilious performance of prescribed duties. It is the duty of an officer to maintain the cheerful and good humoured loyalty of his men. We can, of course, imagine a 'rationalized' version of Army regulations in which it was prescribed that every day a sergeant major should crack a certain number of jokes. It would however be excessive rationalization to embody the prefabricated jokes that he should crack. Clearer concepts of formal and informal organizations are needed in order to resolve the muddles in the theory of 'occupational status'. Some of these are well displayed in Caplow's penetrating review of the major studies. He observes with admirable scientific caution that 'while these scales are reasonably consistent in ranking bank presidents high and street sweepers low, they are not at all consistent in their evaluation of intermediate occupations'. He gives an amusing little table on four selected scales. The ranking of farmers, electricians and insurance agents are respectively as follows: 1, 2, 3; 3, 2, 1; 2, 3, 1; 1, 3, 2. He proceeds to raise a number of thought provoking questions about these scales. 'Where should professional burglars, fortune-tellers, ward keelers, gamblers, prostitutes, and other deviants be classed? Should they be arranged in a hierarchy of skill? How about apprentice gamblers and part-time prostitutes?'

Muddles inevitably come about when people plunge into empirical research without any preliminary classification of concepts. The hierarchies of status have their basis in human 'attitudes' of an immense variety of kinds—dispositions to fear, reverence, admiration and innumerable shades and blends of the simplest of these. Some of these attitudes are formalized or institutionalized in corporate bodies, an army, a church or a civil service. There are many attitudes of respect—for learning, skill, prowess or moral qualities—which are not formalized; and to complicate matters some of these attitudes are bipolar. Hence the puzzle about the burglar and the part-time prostitute.

The details assembled in the chapter on the measurement of occupational status give more than a little support to Pear's recent strictures on the concept of 'socio-economic status'. The term itself confesses to the blending of at least two partially independent variables, and the variables are certainly more than two. Only a skilled hostess can decide upon the order of precedence of say a general, an archbishop, a captain of industry, a world champion boxer and a top level atomic scientist.

All this becomes the more evident in the light of Caplow's illuminating discussion of the 'mobilities'. 'Mobility', he writes, 'is one of the most frequently used words in the sociological vocabulary, and one of the most confusing'. He proceeds to distinguish between 'horizontal' and 'vertical' mobility under each heading to distinguish further between a number of importantly different cases. It will be quite inexcusable if any future discussion of social mobility ignores these distinctions.

An adequate review of this entertaining and instructive work would be almost as long as the work itself. For occupational psychologists it should be more especially entertaining and instructive. They will find after reading it that all these problems have a new look. No vocational psychologist, for example, could fail to get something out of Caplow's discussion of vocational choice taken in conjunction with his chapter on occupation and family. The young and inexperienced vocational adviser might be tempted, in guiding a youth in the choice of a career, to assume that the matter to be decided is one between himself, the boy and his Maker, and that there is no call for incompetent interferences from the boy's parents. The sociologist, however, would teach him that the family is important as a social institution. There is evidence that boys are influenced in their vocational choices by mothers, uncles or uncle surrogates, and fathers (in that order). The vocational psychologist may be a kind of a 'rationalized' uncle surrogate. There are occasions on which a boy should be advised to follow in his father's footsteps, and there are occasions on which he should be advised to strike out on a line of his own. When there is a divergence between the boy's own inclination and the aspirations of his parents on his behalf there is always, as Caplow observes, a risk of intra-psychic conflict whatever he decides.

The *Sociology of Work* should help occupational psychologists to stand back and look at their job from a fresh point of view. It imposes on them new responsibilities since some of the questions it raises are questions which only they can answer.

C. A. MACE

Training Managers in the Public Services. By F. BRAY AND OTHERS. London: Allen and Unwin, 1955. Pp. 84. Price 7*s.* 6*d.*

Looking back over the last 15 years it is possible to think of the problems facing industrial psychologists in the personnel field as coming in successive waves. First there was operative training, then supervisory training; and next, with an increasing labour shortage, 'Human relations training'; then an excursion into problems of joint consultation, and, linked with that, the still fashionable 'communications training'. Now training is being aimed at the higher ranks of industry, for Management training, or executive development (as it is sometimes rather pompously called) is the order of the day. Maybe in another five years the topic will be the training of directors. Parallel with those has come the development of new techniques of training—T.W.I., visual aids, films and film strips, group and syndicate discussions, role playing, case studies. So as each new problem has emerged it has drawn on the techniques developed for earlier ones. The young executive of today, ripe for development, is likely to have to face a strenuous course in which he will have to participate (blessed word!) to the full; he may have to chair a conference or act as its secretary; play the part of a disgruntled worker, an ambitious shop steward, or a harassed personnel manager. In some of the more advanced courses he may even be called on to analyse what he felt he was feeling at the time, too. He may have his speech corrected and his prose style dissected, while his retention of what he learns will, it is hoped, be improved by duplicated lecture notes set out according to the approved fashion, by taking part in group discussions, by watching films, and by visual aids varying from a simple chart to a flannel board. As Guttridge said in another connection, "Pity the poor executive".

The Public Services—among them the Post Office, the Ministry of Pensions, the Coal Board and Electricity Authority, the L.C.C. and the Hospital Service—have certainly not lagged behind in developing their training schemes for managers. This short book reviews the work they have done and outlines much of what they hope to do in the future. An appropriate executive for each service outlines the training carried out in his concern, while the book is prefaced with a study of the problems of management training in the Public Services and wound up rather irrelevantly with an essay by

Colonel Urwick on management training in the U.S.A. This last is a concise and useful summary which concludes with the colonel's previous plea for a general staff in the management hierarchy as a nursery for potential directors.

The size of these public concerns has been an advantage in that the numbers to be trained have been, and are, large, and greater facilities are available; thus the Post Office runs a training centre at Clacton-on-Sea in a hotel from September to May (they close down, by the way, over the Christmas rush season) while the Electricity Authority has two training centres (one called 'Electricity Hall') in which it is fully realized that much of the value of training courses comes not so much from the course content as from informal discussions 'on principle and practice . . . in the lounge and bar in a social atmosphere where the truth eventually emerges without the restraints of the formal meeting'.

Generally speaking, all the training courses and facilities for managers in the Public Services seem to reveal adequate, if not excellent, attention to the 'Knowing that' part of management. While imparting 'Knowing how', the real 'art of management' is dealt with at least as much, if not more, enthusiasm and energy as is shown in private industrial organizations. The methods to which we have become accustomed are used, often (as e.g. in the Electricity Authority) with what appears to be considerable skill and insight. Certainly the Public Services have schemes of training of which they are rightly proud while admitting no room for complacency.

But still, a doubt remains. The Post Office, for example, spends £15,000 a year on running its Training Centre and claims that it is not extravagant in either accommodation or staffing. That may well be so—but is that money well spent? Has the training produced results? The Post Office has to admit they have not been able to judge in a precise fashion the measure of 'our success in raising morale, in ensuring economy, and in giving a better service to the public'. But at least they are aware of the need for following up their training in order to estimate its value. So is the Ministry of Pensions and National Insurance, but neither the Coal Board nor the Electricity Authority nor the Hospital Service mention it specifically and the L.C.C. only touch the fringes of it. That is not enough. Far too little attention has been paid to the necessity and to the methods of validating training courses. As a result methods and techniques which seem good on the face of it are taken over without any check on their effectiveness. It may well be that some of the methods are useless, while greater success could be got by less elaborate and less expensive means. This little book, lucid and comprehensive up to a point, would have been of much greater value if it had shown greater awareness of the need to validate training. T. M. HIGHAM

A Handbook of Industrial Welfare. By R. R. HOPKINS. London : Pitman, 1955. Pp. 258. Price 17*s*. 6*d*.

The title of this book correctly describes its contents which consist of facts and comments on every phase of industrial welfare. Mr Hopkins is personnel manager of a large firm which is well-known for its liberal policies and for its support of research in the social sciences. His long and practical experience of welfare work and his wide contacts with others in this field give him every right to speak with authority on this many-sided problem.

In the preface, Mr Hopkins states that "the reader principally in mind has been the works manager of an undertaking employing people numbered in hundreds rather than thousands". He has, in fact, succeeded in writing a book which should appeal to a much wider audience, and it should prove a particularly helpful introductory guide to students of all kinds who wish to make a career in the industrial field.

The term 'welfare' is widely interpreted to cover both its obligatory and voluntary aspects. In the opening chapter, which gives a useful outline of the history and purpose of industrial welfare, the main aspects of a welfare programme are stated to be the legal requirements, economic welfare and social or humanitarian welfare. Throughout, the author stresses the humanitarian approach, but in a practical, non-sentimental way which, while insisting that employers and employees have obligations to each other, places the responsibility for general morale firmly on the shoulders of management.

The next five chapters give factual outlines of industrial legislation, the voluntary associations of employers and employees, and the multitude of welfare schemes which can be considered an established part of modern industrial life and of those which many firms have introduced when circumstances have allowed. Those considering the introduction or extension of welfare services will be particularly interested in Chapters VII and VIII which deal with the organization needed, the types of people to run it and its cost which the author shows to be not prohibitive when the direct return in lower labour turnover, decreased absence and increased efficiency are considered, as well as the indirect return in improved morale.

A final section, "Looking Ahead", discusses the controversial question of the part played by the social scientist in industry and some of the techniques he has introduced. Some may quarrel with the emphasis placed on groups and group discussion but there is little doubt that much dissension and misunderstanding would be prevented if there was more understanding of the interaction of individuals in group situations at all levels.

At many points in this book, the author gives hints on joint consultation which show that he and, by implication, his firm have grasped the subtler psychological factors which make the difference between success and failure. To mention a few only, the stress on preparing workers' representatives beforehand for emergencies, for example, redundancy (p.83); giving them freedom to pursue their duties as representatives (p.84); encouraging them to see the problems of others, for example, local transport undertakings (p.171); giving selected representatives of the workers the responsibility for arbitrating on applications for up-grading (p.101). Many more examples could be quoted but these are an indication of the emphasis placed on participation in and responsibility for decisions by the workers.

No book is perfect and this one might have been improved by direct references to the research work which is mentioned. In the useful chapter on the cost of welfare services, tabular presentation of the data would have made for easier digestion. But these are minor points. This account of industrial welfare is both challenging and encouraging to those who work or hope to work in that field, and it should help to refute the views of those who adopt a cynical attitude or still talk or act in terms of 'paternalism'.

R. MARRIOTT

Die Tests in der klinischen Psychologie. By ERICH STERN. Zurich: Rascher Verlag, 1954. Po. viii+418. Price Swiss Frs. 28.10.

This is the first volume of two which are meant to introduce a number of clinical tests to the German reading public. Twenty-four authors have contributed, some of them German, others Swiss, British, American, and French. The tests discussed in this volume are tests of intelligence and ability, on the one hand, and tests of personality and character on the other. Tests such as the Thematic Apperception Test, the Rorschach, and Bühler's World Test apparently are not considered to fall under either of these categories because they are to be discussed in the second volume. In addition to a discussion of tests there is also a chapter on statistical methods including factor analysis, a historical chapter, a chapter on interviewing, and a general introduction to the problems and tasks of clinical psychology.

The main value of the book will be in introducing to the beginner a large number of different techniques, points of view, and methods; he will also be given a variety of references to follow up any interests that may develop. If the reader is at all critical, however, he will notice that the book does not face any of the systematic problems which make this field so difficult. He will find theoretical assumptions made by one author contradicted by another without any attempt to evaluate these assumptions or to discuss them on the basis of factual evidence. He will find the writer of the psychometric chapter advocating methods of proof which are patently disregarded by the writers of other chapters. In fact, he will very likely come to the conclusion that this is a collection of papers of very varying degrees of competence and knowledge which are presented to him without any effort at synthesis.

If the reader is at all sophisticated he will notice that most of the authors have made quite uncritical contributions, praising the particular method which they are describing while leaving out a large volume of critical work which has been published in recent years. This, to the reviewer, seems quite indefensible as it makes the majority of the articles unrepresentative and misleading. To give but one example, the chapter on the Szondi test only gives seven references to writers other than Szondi, only two of which are in English. One of these two is by the author of the chapter himself, the other is Deri's introduction to the Szondi Test. Not a word is said about the thirty or so severely critical studies which have been published in American journals during the last ten years.

It is unfortunate that the first book in German to deal at all exhaustively with modern clinical tests should suffer from such very marked defects. From many points of view it would have been better if it had never been published at all because, as it stands, it is seriously misleading and will give the reader an altogether wrong idea of current work in this field. A few chapters, such as that on the Minnesota Multiphasic Personality Inventory, are models of what contributions to such a book ought to be, but there are not enough of these to make it possible to recommend the book as a whole.

H. J. EYSENCK

English Social Differences. By T. H. PEAR. London: Allen & Unwin, 1955. Pp. 318. Price 18*s*.

In recent years there have been several carefully designed empirical studies of social stratification and social mobility. Their rigorous quantitative approach has contributed greatly to our understanding of social processes, but it necessarily involved operating at a high level of abstraction. People appear in tables and graphs as neatly categorised units, which cannot tell us much about the underlying variety in the lives of men and women on different runs of the social ladder, and the more subtle features of their relationships. It is with these that Professor Pear is mainly concerned. Being one of the few psychologists who refuse to be confined within conventional academic boundaries, he draws his material from a wide range of sources; novels, magazines and newspapers are used in addition to research reports and personal observations.

The main part of the book is taken up by a description of the changing attributes of classes and strata in English society; education, leisure, pursuits, manners and etiquette are dealt with in an unorthodox and often humorous way. Perhaps the most penetrating discussion is that relating to different kinds of speech, a topic on which Professor Pear writes with special authority, having been one of the pioneers in the field. Many illuminating insights are provided in passing, as a few random examples will illustrate: "Psychiatric social workers find that many parental worries classed as financial are caused by a strong desire that children shall rise in the social scale". "Perhaps one of the most important and least often mentioned aspects of English social stratification is the inferiority of the seller in the buyer-seller relationship". "English social strata have manifest visible and audible signs. To a blind man there are also tactual signs, for example the soft hands of the leisured. To people with sensitive noses class may have olfactory divisions".

If one obtains more glimpses of upper- and middle- than of working-class behaviour and attitudes, it is because working-class life has so far been much less thoroughly explored from this particular angle. There is plenty of 'door-step' information, but most social investigators being middle-class themselves have no relevant experiences as participants. The importance of this gap has become evident in view of the issues out of which the recent railway strike arose.

Another problem of interest to the occupational psychologist, suggested by this work, is the extent to which degree of adjustment and chances of progress in certain types of job may be influenced by such class variables as speech and manners, quite apart from technical skill. It is certain that such factors are tacitly taken into account by vocational advisers, but to the knowledge of the reviewer they have seldom been fully discussed, still less made the subject of special study. It might be possible, for instance, to investigate how far the possession of too refined or too coarse an accent tends to result in the rejection of a candidate by his fellows in a group selection situation, thereby jeopardising his success.

In conclusion, it may be said that this book is a valuable complement to the existing literature on social stratification. Its merits lie not so much in systematic exposition, as in the wealth of colourful descriptions and stimulating ideas which touch upon one of the fundamental social problems in Britain today. GUSTAV JAHODA

Equal Pay: The Civil Service Scheme. Some First Thoughts on its Implications for Industry and Commerce. By E. MARJORIE HARRIS. London: Institute of Personnel Management, 1955. Pp. 27. Price 4*s*.

The Civil Service scheme to introduce equal rates of pay for men and women performing equal work was introduced in January 1955, and it is intended to be fully implemented by January 1961. Britain can hardly be said to have been in the forefront of the movement towards sex equality. The United States Civil Service accepted the challenge 85 years ago, and the Soviet Union, rather a latecomer, joined the party as long ago as 1936. Those who feel that our national prestige is sullied by this tardiness can at least be grateful to the London County Council for its decision to apply equal pay in administrative posts 10 years before the U.S.S.R. Nevertheless the Treasury decision does represent an important change for this country—a new dawn, or the thin edge of the wedge, as you prefer. When an organisation employing some 200,000 women in various parts of the country makes a radical change in its wages policy—a change which will cost nearly thirteen million pounds a year in increased salaries by the time it is in full operation, and which will immediately affect some 130,000 women office workers—it seems reasonable, if rather belated, for industry and commerce to start thinking what effect this is going to have *chez eux*.

The I.P.M. is to be congratulated therefore for producing this brief paper setting out some of the problems which are likely to be raised. It is made up from the contributions to a meeting held in February this year at which a Treasury official presented an outline of the Civil Service scheme, and personnel managers from manufacturing industry, public corporations, retail distribution, banking, and local government discussed the general issues and attempted a certain amount of crystal gazing into the future.

Some, at least, of the contributors seem inclined to take the line that the changes are likely to be relatively limited, because there is already—on the whole—a fairly clear distinction between "women's work" and "men's work" in industry, and hence—on the whole—women don't do "equal" work. This is an easy way out, but hardly compatible with what the champions of equal pay appear to have had in mind originally. It will be a pity if the campaign for equality achieves no more than a rigid system of demarcation between the jobs open to members of the opposite sexes.

The papers are lucidly presented and well worth the attention of industrialists who prefer to formulate personnel policies in advance rather than allow themselves to be swept along by the tide of events. JOHN D. HANDYSIDE

Revising a Merit-Rating Scheme. By A. G. P. ELLIOTT. London: Institute of Personnel Management, 1955. Pp. 24. Price 3*s*.

In this short pamphlet, one in the series of occasional papers of the Institute of Personnel Management, Mr A. G. P. Elliott tells us why and how he revised his company's merit-rating scheme. As it is hoped that the paper will be of particular interest to personnel managers, it seems reasonable to consider both how useful they will find it and how sound is the advice it contains.

Mr Elliott has described clearly and concisely how analysis of the old procedure disclosed high correlations between ratings on the nine traits used, and factorial analysis indicated that "on almost every item the merit-rating scheme was measuring more of a general factor than of the specific quality involved". Concluding that this general factor can be identified with halo effect, Mr Elliott thereupon determined to adopt a scheme which would make use of it rather than minimise it. After very brief discussion of various available techniques he plumps for the method of paired comparisons, and the rest of the pamphlet describes in fair detail the essentials of this method and how it was received by raters. It is always interesting to read an account of work done in his own field by a specialist, and no doubt in this case the work was well done and suitable in its setting. From the point of view of the personnel manager, however, and particularly one who has not a great deal of experience in this matter, the paper seems to be misleading in several respects. It is perhaps not altogether Mr Elliott's fault; he may have been persuaded to pack too much into less than twenty pages. But as it turns out there are a number of propositions asserted which cannot be accepted without qualification.

In the first place, it is not legitimate to equate 'general factor' with 'halo effect', as is implied in the sentence, "Even in this attenuated form the scales are likely to be swamped with a general factor (or halo effect, to use psychological jargon)" (page 7). It is perfectly possible to find a general factor which is not a halo effect, and indeed if the traits to be rated are all aspects of the same whole, as for example, height, length of leg, girth, length of arm are aspects of stature, it is to be expected. This is not to say that no halo effect was present in the case under discussion; but it is a clear misuse of technical terms to imply that general factor=halo effect, and can do nothing but harm.

Secondly, the statement, "We know that statistically the average of a number of subjective judgments is likely to be more accurate than most of the individual assessments, and that the more judges there are the nearer the truth the average will be" cannot be made without qualification. Sometimes this will be true, sometimes not, depending on how good the judges are. If A is a good judge of men and B is not, then a combination of their opinions about a group of workpeople will be less valid than the opinions of A alone. Again, this is not to deny that a combination of three judgments may be suitable in the circumstances of Mr Elliott's scheme, but to make the complaint that a categorical assertion like that just quoted is thoroughly misleading.

Thirdly, in a paragraph on page 21 there is reported a greater consistency of ranking by junior executives than by supervisors, and even more consistency by senior executives, as judged by how well the various rankings agree with the final, combined ranking. Mr Elliott comments, "Now this is in contradiction to popular mythology; it is generally believed that a man's immediate supervisor is the right person to assess him because he has intimate knowledge of his work. If, however, we think for a moment about the nature of halo we can see that the finding is plausible: the executive is less likely to be influenced by intimate knowledge of the man and his personality and may be able to make a more dispassionate judgment on his work." The suggestion is that the rankings of the executives are less affected by halo. But since the whole scheme is "to accept the inevitable and use the halo factor as our gauge of ability", surely the argument is the wrong way round. By agreeing most closely with the final combined rankings, Mr Elliott's executives are most affected by halo and his supervisors least. Thus the contradiction disappears; the immediate supervisor is indicated as the right person to assess his workers, and popular mythology is shown to agree, on this occasion at least, with the results reported by investigators in this field.

These criticisms have been elaborated because the points put forward by Mr Elliott seem to be likely to mislead the reader inexperienced in this field. They detract a great deal from what is valuable as a straightforward description of work done. In addition, a serious omission must be noted: no explanation of the object of the merit-rating procedure is given.

D. C. S. WILLIAMS

Mental Health and Human Relations in Industry. Edited by T. M. LING. London: H. K. Lewis, 1954. Pp. xix+265. Price 21*s.*

Dr Ling, in addition to editing the contributed chapters of eight others, has also provided four chapters of this book himself. In one of these chapters he says (pp. 208-9), "As regards management training, there are three outstanding needs: first, the psychological power of human understanding and leadership; secondly, what the French call *'le sens du practicable'*—a proper understanding of the logistics of industrial enterprise; thirdly, a capacity to appreciate and apply scientific knowledge. This book is primarily concerned with the first-named, that is, with the human problems of industry or 'Human Relations in Industry', as the subject is widely called." There is little doubt that the book is addressed mainly to managers, and the emphases are therefore quite logically ranked in the descending order quoted above.

The best feature of this book is the relative straightforwardness of its language; the worst feature is the widely-scattered evidence of hasty preparation for the press, of which a few examples will be given shortly. The level of contribution varies widely, from a first-class chapter on "Research in Industrial Management" by J. F. Scott, to pedestrian and platitudinous offerings from people who shall be nameless but of whom the reviewer thought better. Despite the presence of two well-qualified psychologists among the contributors (H. G. Maule and V. W. Wilson) industrial psychology does not seem to get anything like a square deal if one is to judge from the remarks of both the medical officer and the personnel officer which have apparently passed editorial scrutiny. On p. 135 the former, speaking of what he calls 'the health team' and its task, says it should "see new employees and make an endeavour to assess their physical *and mental attributes and abilities,* so that jobs are not filled by those who are unfitted for them" (reviewer's italics). On p. 150, the latter (the personnel officer) states, "To some extent selection is a 'service' in that the personnel department, with the help of the medical officer, should be able to make a general decision on whether a person is employable in the firm or not". True, he goes on to say, "In a large department, there may be one or more people trained in administering and interpreting (*sic*) intelligence and aptitude tests", and refers the reader to one of Wilson's chapters. Neither contributor mentions the role of the psychologist explicitly, or takes the opportunity to identify him as a welcome professional colleague. How very far we still have to go!

Wilson (pp. 97-99) and Ling (in various places) refer to the problems associated with the term 'leadership', but surprisingly give no detailed attention to research findings of the last ten years nor to the important implications of the usually 'negative' results. Wilson does, however, provide a readable and discursive commentary on the need to translate vaguely abstract 'qualities' into something behavioural and hence more concrete.

Maule's chapter on "The Working Environment" is an admirably lucid and well-ordered summary of our firm knowledge in this field, and it is fervently to be hoped that reputedly hard-headed businessmen interested solely in facts will at long last be encouraged by this chapter to make use of them. The attention of academic psychologists is also drawn to this chapter, in the hope that its contents may be applied to the working environment in their own offices and departments as opportunity may afford.

Considerable respect for Dr Roger Tredgold's person and achievements must not be allowed to prevent a lift of the reviewer's eyebrows when confronted with the unqualified assumption that psychiatry is a science (p. 165) or with the doubtful claim that psychiatrists are specially qualified to assist industry through research and through selection procedures. Dr Tredgold is better aware than most psychiatrists that their training is woefully lacking so far as research methodology and philosophy is concerned; and their contribution to occupational selection has been shown not to extend beyond the negative function of "screening out the unfit". Other approaches show greater validity and reliability where "choosing among the normal" is concerned.

The evidence of hasty preparation referred to above must now be regretfully adduced. On pp. 58-59 the textual material is difficult to reconcile with the figure to which it refers; on pp. 110-111 there is an obvious confusion between the textual references when relating them to the cited figure. On page 138 one author quotes the tag as ". . . devil a saint would be", but on page 164 another author is permitted editorially to prefer ". . . devil a monk would be".

Scott's excellently-written chapter is allowed to contain one meaningful reference (p. 190) to Newton's reflections on the behaviour of unsupported objects and one (p. 187) which actually refers to "Newton's discovery that an unsupported object falls to the ground"! Finally, various references to names, to departments and to research results known personally to the reviewer are inaccurate or sufficiently incomplete to risk being misleading.

It is recognised that this has tended to be a rather harshly critical review, which no one regrets more than the reviewer. With no desire to make amends for this, but simply because the book honestly justifies it, let it be said in conclusion that, in spite of some defects in content and preparation, there is a great deal of valuable and well-stated material here. Managers, foremen, trade union officials and many others should read it thoughtfully, bearing in mind its omissions, inaccuracies and questionable emphases, but preserving to the point of active use the sound and sane advice it offers. Psychologists working in the occupational field can profit in a different way: to them this book is an unintended challenge. ALASTAIR HERON

Industrial Conflict. Edited by ARTHUR KORNHAUSER, ROBERT DUBIN and ARTHUR M. ROSS. New York: McGraw Hill, 1954. Pp. v+551. Price 45*s*.

The conception and plan of this work merit attention no less than the contents. A team approach is a growing feature of research in most sciences and many would argue that inherent complexity and interdependence make it desirable if not necessary. Certainly, in face of the material which this book has gathered, it would be difficult to deny that the problems of industrial conflict command interest and demand contributions from many branches of knowledge. Nor can there be any question that most of this copious material is relevant both to theory and practice in relation to these problems. To this extent the comprehensiveness of this collaborative effort has been amply justified by results. This is a big book; and it is chock full of facts and viewpoints and 'slants' from half a hundred picked scholars and authorities all bearing on industrial conflict.

However, as another eminent American, Mr Cecil B. de Mille, demonstrated some years ago, a difficulty with the 'colossal production' and the 'all-star cast' is to avoid bewildering and confusing the recipient. The blondes of one moment cancel out the brunettes of the next. One no sooner sees something interesting than something else asks to be followed up. The editors cannot be accused of neglecting this difficulty. On the contrary, they have made a brave effort to achieve 'integration', 'perspective' as well as 'insights' and 'a frame of reference for thinking and research'. Their elaborate plan and method is described in the preface; and must be considered an instructive example of a co-operative approach to research. In spite of this care and effort the difficulties of bringing the material to order are apparent. There does not seem much more of a frame of reference than the plan achieved at the beginning plus the range and scope of the material gathered. The reader will almost certainly be forced to widen his horizon; but the perspective seems less impressive than the insights; and the integration has not been taken very far.

Psychologists may be disappointed that more of the book is not given to their specialty. Among 40 chapters, only chapter 5 (Human Motivations Underlying Industrial Conflict), Chapter 6 (Satisfactions and Deprivations in Industrial Life), and Chapter 28 (Group Dynamics in the Industrial Situation) go to any psychological depth, while Chapter 24 (Basic Employment Relations), Chapter 26 (Work Methods, Working Conditions and Morale), Chapter 13 (The Role of Supervision) and Chapter 29 (Recognition and Utilisation of Employees' Abilities) cover ground which will be familiar to the psychologist working in industry. For this the psychologist should not look to attack the editors, but prepare to stand in defence of his mystery because they have carried the attack to him. In their preface they clearly acknowledge the potential importance of contributions from psychologists, but proceed to lament their absence, with the implication that psychologists have been sidetracked from these important areas of research. The reviewer must confess that working as a psychologist in industry he had felt acutely the limitations of his knowledge and skill and influence in face of industrial conflict, even of industrial unrest; and had felt a corresponding need for just such a frame of reference as this book attempts to provide. Whether the editors have taken all that psychologists can provide at present is perhaps debatable. Motivation is the keynote of the psychological contributions in Chapters 5 and 6. It may be suggested that equivalent selective consideration of perception could be useful. This would chime with the approach and techniques based on communication and to some extent those based on participation. The reviewer remembers that one of the editorial advisory board (Stagner) stressed perception as well as motivation in relation to industrial conflict as recently as 1948 (*Personnel Psychology*, 1, 131-143). It is possible also that the psychological analysis could go deeper and further: sources of anxiety and unrest may be far from a conscious level and may lie deep in the nature of work and organisation in modern industry and life in a modern society.

One important factor that does not emerge clearly enough from this book is the dissociation between trade unions as institutions and the individuals and informal groups that are their constituents. Criticism on this point cannot be pressed very far because the book is concerned with industrial conflict in U.S.A., and there this dissociation is not so evident as in Britain. 'Wildcat' (unofficial) strikes are mentioned only in one chapter; and in general the institutional context of conflict is emphasised. In Britain and in many other industrial communities unofficial strikes compel much more attention to the informal psychological and sociological element of industrial conflict; and the dissociation referred to tends to emerge as a major variable in the situation. With this one addition the reviewer believes that a frame of reference suitable for the psychologist working in industry could be worked out of the material in this book.

However, criticism on either of these lines does not diminish the very positive value of the book. An enormous amount has gone into it; and it will yield corresponding knowledge to the determined reader. The most experienced thinkers and workers on industrial conflict should find it highly stimulating and a little chastening in the scope it takes as relevant to the subject. The reviewer can testify that if it had been available several years ago it would have saved him much laborious research and some painful trial and error.

E. P. KELSALL

The Works Manager and his Responsibilities. Report of a Membership Survey. London: Institution of Works Managers, 67-68 Chandos Place, W.C.2, 1954. Pp. 12. Free on application to the Secretary of the Institution.

The Council of the Institution of Works Managers instructed its Education and Research Committee to circulate members with a questionnaire. It had four ends in view:

(1) to plan its activities in the light of greater knowledge of the needs of its members;

(2) to be in a better position to press for due recognition of the functions of works management;

(3) to enable it to meet the educational requirements of the potential works manager;

(4) to provide the individual member with a yardstick by which he could assess his own position in regard to status, responsibilities and salary.

After a pilot run by one branch, a postal questionnaire was issued to the 1,600 corporate members of the Institution and 533 replies were received. This 33 per cent sample was checked and found to be a fair sample of the Institution's membership. The report gives an analysis of the replies and is admirable for its brevity and for its clarity. It is only twelve pages long, including several tables and figures, and yet gives a lively and critical account of the position of works managers according to the replies.

The first two questions were framed to discover whether the membership of the Institution was representative of British industry in general, both by industry and by size of firm. The answers suggested that it was, on the whole, representative by industry, but that the larger firms were over represented. The next three questions were on the positions held by members, the salaries they earned and the functions they controlled. A third of the members were described as 'Directors' or 'General Managers', but half of this third controlled establishments of under 250 people. ". . . and this is a good sign" the report says, "because it means that top executives from small firms are joining the Institution and finding value in it". The salaries earned by members are tabulated in different ways, but a summary of particular interest and ingenuity is a diagram showing the medians and quartiles of salary by main status groups, ('Directors', 'General and Works Managers' and 'others') according to the number of persons under the jurisdiction of the individual.

Some of the answers to the question " What functions are controlled by members of the Institution?" are surprising. Out of a list of sixteen functions, "Personnel" ranks equal top with "Operation of Processes" with 98 per cent of Works Managers having a controlling function. However, in these two cases only 56 per cent and 47 per cent respectively of those answering said that they were normally consulted on policy regarding these functions. The report points out this discrepancy, ". . . in too many cases Works Managers are not being consulted in the planning of policy that covers the functions they control. Efficiency must suffer where policy is framed without intimate knowledge of circumstances in which it must be applied. Moreover, those who have used Joint Consultation with success at lower levels know the benefits that accrue when those who have to implement decisions have been present at the discussion, or better still, when they have shared the responsibility for such decisions".

Works managers will find this short report a stimulating change: for once they will be able to read about their own conditions and problems instead of studying the innumerable books about the people for whom they are responsible. If other Institutions made similar enquiries and reported on them as excellently, subscriptions would be paid with better grace.

WINIFRED RAPHAEL

Science and Social Action. By W. J. H. SPROTT. London: Watts, 1954. Pp. iv+164. Price 15*s*.

This book might well be taken as compulsory reading for students of sociology and social psychology. It is written with the lucidity and humour characteristic of its author, but it demands and deserves close attention throughout, for it is mercilessly packed with argument. It is in effect a cautionary tale that Sprott has to tell, and one that might be profitably read together with Farrell's recent article " On the limits of experimental psychology" (*British Journal of Psychology*, 46). Both writers stress the present-day lack or inadequacy of 'high level' theories in psychology, and Sprott seems almost to despair of obtaining them in the sphere with which he deals. Such theories wait for the further development of methodology, including systematic analysis of the fundamental concepts involved. Perhaps, however, we might say that even in the more exact sciences the intimations of immortality for theories have not proved altogether reliable. Of one thing the writer is quite sure, that the lack is not to be made good by translating the plain statements of the market-place into a pseudo-scientific jargon derived illicitly from other disciplines. When he bluntly termed such pretentious formulations humbug the reviewer's heart warmed greatly to him.

Not only have we to admit the absence of unifying theories which would illuminate the low-level findings of restricted research, but even the latter remain vulnerable, since they can only apply strictly to the context in which they were discovered, inquiry is difficult by reason of the intricacy of the factors operating, adequate sampling is expensive, and the available statistics incomplete. Stating these points is not an expression of defeatism, but rather a demand for prudence and modesty in assessing results. Those results may often, as the author says, be trivial and not surprising, nevertheless there is profit in having evidence for familiar beliefs, evidence which can be judged and challenged. It may well be that man long ago in some way or other reached the most important truths about society, yet science rightly demands clear evidence for even the most obvious truths, for only so can we hope to reveal more ultimate principles.

The first chapter provides an excellent introduction to the book. Science and its history are part of social history, and at all stages determined by the current pattern of society, and though, as he points out, discoveries are made by individuals and not by groups, yet the individuals are directed by the social system in which they are imbedded. "A social system is a system of positions and roles, each of which has certain standards of performance and attitudes attached to it." The concept behind this definition is that of social action, which consists of the mutual adaptation of individual behaviour. From that arises a system of reciprocal expectations, and thus a group comes into being. Sprott has little patience with 'natural human urges' to this or that, holding that social psychology can find a sufficient basis in the observable facts to which he has appealed. It is not possible to summarise this series of lectures, so we will just refer with special commendation to those on Assimilation and Deviance. Caution and modesty together with incisiveness are shown throughout the book: the illustrative examples chosen are interesting and provide good material for analysis, and the references are ample. In short, this is a valuable addition to the series of Josiah Mason Lectures delivered in the University of Birmingham. At the end the reader may not know much *more* (though almost certainly he will) but he should know *better*.

A. W. WOLTERS

Working-Class Anti-Semite: *A Psychological Study in a London Borough*. By JAMES H. ROBB. London: Tavistock Publications, 1954. Pp. xiv+233. Price 15*s*.

It is encouraging to people who are concerned about anti-Semitism that there should have been so much agreement in the findings of recent researches, now on both sides of the Atlantic, about the typical personality structure and circumstances of the anti-Semite. Dr Robb has built upon previous researches, which are summarised with admirable conciseness in the present book, in designing his own investigation in such a way as to test specific hypotheses: namely, that the anti-Semite is a paranoid, pessimistic sort of person, with fears about his ability to control the environment, anti-authority if he is a leader, otherwise submissive, who has in fact experienced a number of anxiety-creating situations and who shows tendencies towards isolation. Dr Robb's methods of investigation are basically impressionistic, in that his evidence about both anti-Semitism and its correlates are obtained from interviews and the Rorschach test, but the interview material is treated by means of rating scales, permitting statistical tests. All his hypotheses are confirmed except that concerning paranoid tendencies, which has prompted an interesting discussion on a possible distinction to be drawn between paranoid and projective symptoms.

There is an additional focus of interest in the present study. Since it is a localised inquiry, the book has also sociological value in offering a study of a London borough, Bethnal Green. It was chosen by the writer as a highly homogenous sub-culture, a working-class area (not usually the subject of studies of anti-Semitism), and a borough regarded before the war as strongly anti-Semitic. Included here are both a study of the family and social patterns of Bethnal Green and a historical survey, largely concerned with tracing the actual impact of Jews on the borough. This anchoring of the inquiry has the further merit of enabling the writer, in subsequently sketching his own theory of the way in which anti-Semitism develops, to take into account the contribution of the social environment more than is usually possible. He finds in fact a greater degree of anti-Semitism than has been uncovered by the American studies with which he compares his results, which may merely mean, of course, that there is a greater *concentration* of ethnocentric attitudes in Britain than in America; it is Dr Robb's own view that anti-Semitism is a "particular manifestation of prejudice rather than a unique situation".

Dr Robb concludes with a discussion of methods of alleviating the problem. He states his reasons for rejecting the means of legislation, education and social change, and instead makes two suggestions of his own. The first is the use of the technique of group discussion, directed at the less extreme cases, at 'gatekeepers', as Lewin termed people in positions where they might influence others, and at children. Here the proof of the pudding is in the eating. His second suggestion is that of prevention, "by aiming at a reduction in the development of prejudiced personalities". This seems most certainly to imply the means of social change, which he has previously criticised.

In review it seems astonishing that so much material has gone into this small and inexpensive book. This is achieved by admirable economy of statement. Some criticisms can be made, but they are minor ones. It would seem undesirable for Dr Robb to have analysed the Rorschach material himself, after lengthy interviews with the people concerned, yet there is no evidence of his having used an independent interpreter. This is a minor criticism since in fact he makes little use of the material in the body of the book. The Rorschach protocols are set out in an Appendix (a partial answer to the last criticism) and since this book will certainly be read by a much wider public than one of initiates, it might have been useful to append some explanatory notes leading at least to an understanding of the symbols used, although not the ability to interpret from them. Further, Dr Robb is not altogether explicit about his reason for confining the study to males. It was perhaps because other studies have shown women to be less anti-Semitic than men, but in view of his recommendations that attention should be devoted to the *less* prejudiced, and to 'gatekeepers' (which mothers certainly are towards children), he should perhaps have included in his suggestions for further research a study of the *women* of Bethnal Green. Finally, there is an undue amount of apology and explanation for what has and has not been done in this research, the mark of the thesis on which the book is based. This is particularly unnecessary as one cannot but be impressed throughout by the writer's courage in choosing to tackle a problem of importance but methodological difficulty; in conducting it not on University students but in Bethnal Green, to which he came as a New Zealander first to work as a barman in order to study its culture patterns, then to conduct house-to-house interviews, often on the doorstep; in attempting to advance the usual level of theorising on this problem; and in his forthright statement of practical remedies for dealing with it.

THELMA VENESS

Morale in War and Work. By T. T. PATERSON. London: Max Parrish, 1955. Pp. 256. Price 18*s*.

In this book the author describes an experiment he carried out at an airfield in the West of England during the war. The airfield had a very high accident rate and the author, on being asked by the Station Commander to "do something about it", early came to the conclusion that despite its dreary surroundings (the station is called Bogfield in this book) the problem to be dealt with was primarily psychological. What was necessary was not so much to heighten morale, for morale was high, but to bring about a change of almost equal importance—to provide a clear goal and greater sense of interdependence or team spirit.

Morale is here taken as a strong sense of duty towards a group ideal which finds its expression in a disciplined code of behaviour, but is satisfied only through co-operation with others towards the fulfilment of his ideal; hence the vital need for a sense of 'interdependence'. Two basic drives of the participant must be satisfied. In the first place—and this is the more important of the two—he must feel that he is adequately playing a necessary part in furthering the group's activities; that is, he must satisfy his 'Service Drive'. Secondly, he must feel individual satisfaction from the admiration of others for the way in which he had excelled himself—his 'Prestige Drive'. The Service Drive is the more altruistic, the Prestige Drive the more self-centred, but they both depend to a great extent on an adequate relationship ('team spirit' or 'sense of interdependence') between the members of the group.

At Bogfield relations between the various groups had soured. The pilots, stationed as they were in a part of the country where the enemy was rarely to be seen, had not justified themselves in their prime function—that of battle—and, as a result of this frustration of the Service Drive, had become actively aggressive against the other groups with whom they were supposed to collaborate. Their sense of community with these other groups being lost they had become primarily motivated by the frankly egoistic Prestige Drive. Since they had not shown themselves to be fighters, they felt they were of no use in the second of the two pilot duties, managing the aircraft. That too became a purposeless quest: one felt 'bound to prang'.

What was needed to restore self-confidence was to instil a feeling of accomplishment and success; and this required a clear, realizable goal. One of the main reasons for their never getting to grips with the enemy was the very erratic weather conditions which made flying difficult and provided any marauder there might be with easy cover and escape. The author decided to use this drawback as a challenge. If they could not fight the enemy at least they could fight the weather, and thus, through 'Symbolic Conflict' bring meaningfulness to the function of pilot.

But this in turn must be supplemented by a sense of community among the members of the group in the attainment of the goal. A team spirit is necessary, born of an awareness that others are as motivated as oneself and perpetuated by mutual recognition of the importance of each other's task.

This community in a common purpose not merely between pilots, but also between pilots and ground staff, was achieved by encouraging the various groups to meet and explain their jobs and talk over their problems with one another, so that they came to understand the importance of each other's function and that 'the other fellow cared as well'. Later, as a result of this greater friendliness, practice interceptions and landings in the link trainer were conducted between the pilots and their ground 'controllers'. In this and other ways team spirit was built up and each member, aware as he was of the interest of others in his performance, took care to conform to the accepted pattern or 'norm' of behaviour required. In particular, and this was the desired result of the experiment, the pilots were less erratic in their flying.

Throughout the author felt the prime need of modifying behaviour without implying any doubt on their previous performance; without, that is, questioning their justification for feeling capable of pride in achievement. For this reason only positive instructions, how to fly 'well' rather than how to fly 'safely', how they could excel themselves rather than the mistakes they could make, were stressed. Similarly the utmost pains were taken to convince the pilots and others that this attack on the weather was their own idea. To this end the author approached the informal leaders whose opinions were accepted by others and influenced them by presenting his views as a challenge which they took up, as they thought, on their own initiative. (There is an interesting chapter on the various types of group leadership, and a further book on this subject, 'methectics' or 'the study of participation in group life in terms of bestowed and adopted roles', is promised.)

The experiment was a success, the number of accidents per sortie dropped from over three times to under the number expected for the type of aircraft. How exactly this was brought about, though, is not always made clear. The 'experimental' side is frankly descriptive (the chapter dealing with the later squadrons is particularly ad hoc in its analysis of cause and effect) and detailed criticism of the results is difficult—not least because the role of the experimenter was 'participatory' and much seems to have depended on his and one or two others' enthusiasm. A closer analysis of other stations which did not have so many accidents would have been helpful—but such comparisons have been avoided. The theoretical analysis, in its turn, although vigorous, is apt to be misleading. In particular the distinction between 'prestige' and 'status' and the dependence of the former on the latter is not always made clear. Nonetheless the author proved his point in practice and the thesis of the 'Service Drive' satisfied by a clear goal and team spirit is an important one and is explained in an energetic and prepossessing way. Examples are also given to show the relevance of these contentions to industry as well as to the armed services.

ROGER HOLMES

Vocational Aspect of Secondary and Further Education. Editors: A. J. JENKINSON and W. A. SKINNER. Published twice yearly by the Editors from the Training Colleges at Bolton, London (Garnet College) and Huddersfield. Price 5*s*.

The McNair report (1944) states that although some of the best teachers in the whole field of education are to be found in technical colleges and schools, nevertheless . . . "the professional equipment of technical teachers as a whole is not satisfactory and the full benefits of technical education are not being realised because for one reason or another the teaching is not as good as it ought to be". The McNair report went on to recommend the provision of facilities whereby persons with technical skill or knowledge wishing to enter technical teaching might receive training. The staff of the three colleges which were established as a result of the Report decided to publish this Journal and thus to fill a very noticeable gap in educational literature. *Vocational Aspect* describes itself as being "devoted to the vocational element in secondary and further education and aims at elucidating the principles underlying this range of education and examining its history and the organisation now supporting it and at describing and discussing the teaching methods appropriate to it. . . ." It welcomes learning but on condition "that it is assimilable by normal busy teachers".

The Journal seems fairly accurately to cover the ground that it aims to cover and the current issue (Number 14) contains nine articles on such widely diverse subjects as

"Embroidery: Sources of design", "Woodwork for Girls", "The Technical College Library", and "The Planning and Equipping of Engineering Teaching Laboratories". The articles are, as is to be expected, primarily of interest to practising teachers and especially to teachers of vocational subjects, but those on "the quality of technical teaching; some aspects of teaching method" and "the Teacher—Student—Patient relationship in the teaching of professional skills" are of more general interest. The latter title really means what is says for the article deals—somewhat surprisingly perhaps—with the training of chiropodists. It discusses the problems which arise when the student first gets to work on a patient: for instance, because of tension he may lose some of the dexterity he has acquired as a result of practising on apples, potatoes, and oranges. It is suggested that the first contact with patients should be of a social nature—entering name and address, helping to and from chairs, and so on. Emphasis is placed on the importance of keeping the patient in the picture and the re-establishing of student-patient contact after it may have been broken due to discussion of the case between teacher and student. Although this particular article deals with a highly specialised subject nevertheless its general principles may well be pondered upon by many technical teachers.

The Journal as a whole is well balanced and its learning should be "assimilable by the busy teacher". It is to be hoped that technical teachers are not too busy to read it.

W. C. KING

Psychological Testing. By ANNE ANASTASI. New York: Macmillan, 1954. Pp. xiii+682. Price 47*s*.

Professor Anastasi's aim has been "to provide an introduction to the principles of psychological testing . . . to acquaint the reader with the major types of test in current use . . . (and) to prepare the reader for the proper evaluation of psychological tests and interpretation of test results". In this she has succeeded very well. Perhaps she is just a little unsatisfactory on factor analysis, homogeneity and percentiles, and has a bee in her bonnet about differential aptitude tests; but it is clear that she is a real expert at her job and at teaching it to others. The reviewer does not know any other book on testing which he would recommend more confidently than this, though he would advise psychologists in the occupational field to study also Super's *Appraising Vocational Fitness* (Harper, 1949).

ALEC RODGER

Annual Review of Psychology, Volume 6, 1955. Edited by C. P. STONE in association with Q. McNemar. Stanford: Annual Reviews, Inc., 1955. Pp. vi.+517 Price 60*s*.

At the end of a preface initialled by the whole editorial committee, we are told that the aim of the *Annual Review* is "to disseminate the fruits of research in psychology and to promote collaboration among scholars of all nations where pure psychology and its applications are vital to grass-roots organisations devoted to popular education and human welfare, and to programmes of the learned societies, libraries, colleges, and universities". However, if we decide to move on undeterred, we shall find that none of the writers of the eighteen chapters seems specially worried about all this. For the most part, they merely present interesting and fairly detached surveys of their particular fields of study. As usual, of course, some carry their detachment rather far. G. J. Thomas, for example, who writes on Vision, does little more than offer summaries of a lot of investigations. But there are others, including R. M. W. Travers, who deals with Individual Differences, and J. Nuttin, who handles Personality, who both summarise and discuss.

This year's chapter on Industrial Psychology is contributed by S. R. Wallace, Jr., and J. Weitz, of the Life Insurance Agency Management Association, Hartford, Connecticut. They begin by referring to some very critical remarks made by L. H. Lanier (*Psychological Bulletin*, 1954, 51, 180-190) about the corresponding chapters in the first four volumes of the *Annual Review*, in which he spoke of 'inadequate samples, uncontrolled conditions, and uncertain criteria'. They maintain that until industry allows itself to become a laboratory these conditions are likely to exist. Maybe. But is industry ever likely to do what they want on a big enough scale, and continuously enough, to achieve the ends they have in view? There seems to be a touch of unrealism about the authors' suggestions here, as there is in some of their comments on the criterion problem specifically, where, despite their thoughtfulness, they appear to overlook the fact that for certain purposes it is only sensible for the psychologist to take the industrialist's own criteria into account, because these are in fact the things he goes on in running his business. It is not usually, as the authors suggest, a question of 'accepting the criterion at hand and hoping it will turn out all right': it is, rather, a question of seeing what you can do with what you have got, and at the same time trying to educate your user to want something better.

Still, this is a good chapter. In fact, in some ways it is the best we have had on Industrial Psychology in the *Annual Review*. But perhaps the present reviewer's judgment of it has been a little influenced by the attention it gives to the work of British investigators, notably to Handyside and Duncan's 1954 paper in this journal on job satisfaction.

ALEC RODGER

Other Books Received

Company Experience in Improving Office Administration. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1954. Pp. 56. Price $1.75.

Co-ordination and Communication Problems of the Financial Executive. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1954. Pp. 40. Price $1.75.

Current Practice in the Development of Management Personnel. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1955. Pp. 36. Price $1.75.

Electronic Data Processing in Industry: A Case Book of Management Experience. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1955. Pp. 258. Price $7.75. (A.M.A. Members $5.75).

Guides to Effective Production Management: Communication—Co-ordination—Controls. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1954. Pp. 48. Price $1.75.

Searchlight on Office Cost Control. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1954. Pp. 48. Price $1.75.

Tested Approaches to Cutting Production Costs. By AMERICAN MANAGEMENT ASSOCIATION. New York: A.M.A., 1955. Pp. 126. Price $3.75.

Fantasies and Facts in Corporations By CALIFORNIA INSTITUTE OF TECHNOLOGY. Pasadena: C.I.T., 1955. Pp. 22. Price $1.00.

Textbook of Occupational Therapy, with Chief Reference to Psychological Medicine. By EAMON N. M. O'SULLIVAN. London: H. K. Lewis, 1955. Pp. x+320. Price 21*s.*

Ganzheitspsychologie und Strukturtheorie: Zehn Abhandlungen zur Psychologie und philosophischen Antropologie. By ALBERT WELLEK. Bern: Francke, 1955. Pp. 258. Price S.Fr. 21. 50 gb. 18—br.

The Business Enterprise as a Subject for Research. By HOWARD R. BOWEN. New York: Social Science Research Council, 1955. Pp. viii, 104. Price $1.25.

Absence from Work: Recording and Analysis. By BRITISH INSTITUTE OF MANAGEMENT. London: B.I.M., 1955. Pp. viii, 24. Price 2*s.* 6*d.*

Verso la Guista Strada. Le Basi dell'Orientamento Professionale. By LEONE CIMATTI. Rome: Arte Grafica Romana, 1955. Pp. 152. Price L.400.

New Jobs for Old Workers: an Examination of the Statistical Evidence for the Provision of Alternative Occupations. By F. LE GROS CLARK and AGNES DUNNE. London: Nuffield Foundation, 1955. Pp. 20. A limited number of copies free of charge.

Personnel Management: a Human Relations Approach. By WILLIAM H. KNOWLES. New York: American Book Co., 1955. Pp. xvi, 488. Price $4.50.

Kant. By S. KORNER. London: Penguin Books, 1955. Pp. 230. Price 2*s.* 6*d.*

Behavior in a Selling Group: A Case Study of Interpersonal Relations in a Department Store. By GEORGE F. F. LOMBARD. Boston: Harvard Business School (London: Bailey Bros. and Swinfen), 1955. Pp. xx, 360. Price $4.00, 32*s.*

L'Evolution du Travail Ouvrier aux Usines Renault. By ALAIN TOURAINE. Paris: Centre National de la Recherche Scientifique, 1955. Pp. 202. Price 1,200 frs.

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